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*SOME PROBLEMS IN THE LIFE HISTORY OF PATHOGENIC MICROORGANISMS.**

OUR knowledge of the profound influence which the microscopic organisms, known as the bacteria, exercise in the life of the globe, may be considered an acquisition of the last quarter century. The surmises and hypotheses of the half century preceding were then made over into well-attested facts.

The activities of microorganisms manifest themselves in many different ways. The functions carried on by the bacteria of the soil are known to be of fundamental importance to higher plant life. The work of the bacteria producing fermentation, putrefaction and decay is of similar importance in preparing the way for the soil bacteria and ministering to the wants of higher organisms. Out of this latter class there has arisen a group which has given these microorganisms all the notoriety they possess. It is a small group, but formidable in that it is in partial opposition to the higher forms of vegetable and animal life. It is these parasitic forms to which I shall devote my address, as it is they which have preoccupied my attention for some years. In thus passing over large groups of bacteria I simply register my inability to properly present their claims, and I trust that others here present will fully supplement my paper by dealing with them in deserving fashion.

While bacteriology, strictly speaking,

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deals only with a fairly well-defined group of unicellular plant-like forms standing near the limit of microscopic vision, medical bacteriology has been gradually widening its scope to a study of all unicellular and even higher parasitic forms, which multiply more or less indefinitely and continuously for a time in the invaded body. In addition to the bacteria proper, the protozoa, and those highly important ultramicroscopic organisms which seem to have certain characters not possessed by either of the other two groups, are now frequently gathered into medical bacteriology, because of certain underlying principles of action which they possess in common as parasites.

Bacteriology differs from the older sections of biology in several important particulars. In the first place, it has been developed under the stress of practical demands. The enormous economic and sanitary significance of bacterial life has pushed forward this study very rapidly, and the problems undertaken have been suggested almost wholly by considerations arising in agriculture and medical practise.

In the second place, bacteriology, at least so far as the parasitic forms are concerned, is essentially a study of two realms, that of the parasite and that of the host, of two organizations, widely different, acting upon one another and entering into complex, reciprocal relations. The older departments of biology do not present such a complicated aspect. Thus anatomy or morphology has, at least until very recently, dealt with structure and development without considering the relation of the individual to its environment. That was relegated to physiology and pathology. With the bacteria the morphologic aspect dropped nearly out of sight because of the difficulty encountered in analyzing structures so minute and relatively simple. Even the classification gradually evolved, as more and more forms were examined, is at present very largely a physiologic one, the characters being based on the action which the bacteria exert upon the medium in which they multiply.

Then again, there was no ulterior interest in the study of bacteria as such, which is a strong impulse in many other departments of biologic science. It is what bacteria do rather than what they are, that commanded attention, since our interest centers in the host rather than in the parasite. This tendency manifested itself in a peculiar way. As soon as bacteria could be handled in pure culture, the study prosecuted most actively was how most quickly to destroy them. Disinfection, sterilization and all agents which act destructively upon bacteria were diligently sought for. The first impulse of the youthful branch of bacteriology was thus to destroy, rather than to study and analyze. When, some years later, the antibodies were discovered, the rush toward the bactericidal serums was equally manifest.

Bacteriology in its scientific form was thus ushered into existence largely by medical men who had definite practical ends in view. It presented from its beginnings a dual aspect for study and its chief aim from the first was the destruction of one of the elements, the parasite. Slowly, however, the more impartial study of host and parasite in their mutual relation began to take root and to-day there is scarcely a department of physical, chemic and biologic science which does not have some share in the unfolding of this complex relation existing between plant and animal life, on the one hand, and the microorganisms acting as parasites, on the other. As a result of this rather unique state of affairs, bacteriology is not a self-contained, well-defined field of work, but one greatly subdivided by aims and methods of study. A realm as large as that of microorganisms

may well claim attention in many workshops of science.

The short time at my disposal does not permit a wide survey of the field of bacteriology, and I have deemed it best to discuss in a general way the parasitism of bacteria and to outline the probable results of any attempts of medical and sanitary science to modify this parasitism. In undertaking this task I have adopted the somewhat discredited method of presenting actual hypotheses, partly new, partly old, in a new dress. These furnish a definite point of attack, and are better suited for discussion than any presentation which boxed the compass with the views already well known.

Infectious diseases have frequently been portrayed as a battle between two organisms, the host, on the one hand, the parasite, on the other. There are few diseases even among those not strictly infectious in character in which this battle does not go on at some stage and in which the activity of bacteria may be ignored. For some years the analysis of this warfare has been the chief problem of bacteriology and pathology. What are the weapons of offense and defense on either side? Are the weapons simple or complex? Are they changed as the struggle progresses to suit the immediate state of the battle? Do the combatants themselves change during long or short periods of time, and does the character of the disease change as a consequence? Do the parasites act differently when posing for us in the culture tube than in the animal body? These and other queries may easily be read into the special literature of the last decade.

To realize the great complexity of this struggle we need but to review the gross facts of disease which express themselves in epidemics, on the one hand, in individual disease, on the other. We meet all gradations of severity, from rapid death to a mild transient disturbance, from a disease lasting hours to one lasting fifteen or twenty years, or even longer. Even the simplest generalizations concerning such a varied phenomenon must necessarily be subject to many exceptions, and perhaps gross inaccuracies. This is evident from the heated discussions which have been waged over the humoral and the cellular phenomena, the antitoxic and bactericidal forces of the blood and the phagocytic activities of certain cells, each party to the discussion claiming, at least for a time, that the opponent had no case. Though the brilliant researches of Metchnikoff and Ehrlich, and the fundamental discovery of Behring and Kitasato, have to a certain degree exposed the mechanism of warfare, the exposure is only fragmentary, and the hypothetic reconstructions based on it are leading as usual to further controversy. We do know that no two species of microorganisms carry on the warfare just alike, and that the same parasite finds a somewhat different situation in every host attacked. The problem of the immediate future is to determine where the brilliant discoveries of Metchnikoff, Nuttall, Behring, Bordet, Ehrlich and others belong in the life of each microbe, and to construct for each disease the exact nature of the contest.

In the following pages I do not intend to enter into any discussion concerning the intimate life of bacteria, but simply to point out certain biologic problems which seem to lie on the surface, as it were, and which illustrate the close relation existing between bacteriology and general biology. They have suggested themselves to me from the comparative standpoint, one up to the present but poorly cultivated in medical science.

The researches of Roux, Kitasato and Behring, Van Ermengen and others, have shown that certain species of bacteria secrete toxins during their vegetative period.

These toxins are soluble in the mediums in which these species multiply. Besides these physiologically well-defined poisons, there are others which are closely linked to the body substance of the bacteria, and which have become familiar to us in such well-known substances as tuberculin and mallein. According to the theory of R. Pfeiffer, this second class of poisons is liberated only by the disintegration of the bacteria, and the intoxication of the host, due to its destructive action on the bacilli, is a kind of post-mortem effect of the parasites. Other bodies, the so-called lysins, which act destructively upon red and white corpuscles, have also been demonstrated by Van de Velde and by Ehrlich and his pupils, but their significance in disease is not yet clear.

In the host, on the other hand, during the multiplication of microorganisms, there appear bodies known as antibodies, which have aroused the greatest interest. They neutralize the soluble toxins, agglutinate the invading bacteria and disintegrate them. They also precipitate or coagulate albuminous bodies. Their action is specific, being directed toward the invaders. These are the main weapons which thus far have been found. Are there other offensive and defensive bodies? What course do the bacteria pursue in the presence of the gradually accumulating antibodies of the host? Do they forge new weapons or not?

Professor W. H. Welch in his Huxley lecture presented the theory that the mechanism of the production of antibodies on the part of the invaded host was set in operation by the microorganisms as well, and that various tissue poisons might have their origin in overproduced bacterial receptors thrown off under special stimulation by host substances. This theory implies that bacteria may not unfold all their activities in the culture tube and that the latter give us no reliable clue as to their behavior in the living body.

On this point we may perhaps get some light by a consideration of the plasticity of microorganisms. It has long been known that the pathogenic power of bacteria is reduced gradually in artificial cultures. It is also well known that by a series of inoculations or passages through animals the virulence may be restored, and even raised above the natural level. Bacteria have been gradually accustomed to originally destructive doses of poisons in culture fluids. Very recently it has been shown that they may be gradually trained to multiply in strongly bactericidal serums and to refuse to be clumped in strongly agglutinating serums.

These adaptations persist for a certain time and are transmitted for a limited period, even in culture. In other words, the modifications are more or less gradually acquired and gradually lost. The same is true of the antibodies of the host. The antitoxin circulates in the blood of the horse long after the stimulation by toxins has ceased. In the immunized animal the agglutinating properties do not disappear at once. I am, therefore, inclined to believe that the bacterium freshly removed from its usual environment will, at least for a time, exercise all its functions, provided the special nutritive substances which may be needed to carry on those functions are present.

The theory of Professor Welch would then resolve itself into a question of nutrition. In the body of the host there are certain substances which give rise to special toxins when acted upon by special bacteria. If we could offer these special substances to freshly isolated bacteria there is no reason why the assumed toxin should not be formed. We must, therefore, take into account two possibilities, the adaptation of microbes to originally destructive agencies,

and the production of poisons from specific substances elaborated by the host.

I have entered into this much of detail concerning the mutual relation of microorganisms and host in order to make clear the hypothesis, which it seems to me accounts very well for the general phenomenon of infection. It is that the tendency of all invading microorganisms in their evolution toward a more highly parasitic state is to act solely on the defensive while securing opportunity for multiplication and escape to another host. By tendency I mean a general slow movement through long periods of time. The following data are in its favor:

1. The production of diffusible toxins survives parasitism indefinitely, and is readily brought about in cultures.

2. Where toxin-producing bacteria have become adapted to a definite species, as in diphtheria, the toxin itself acts upon a number of different species. In other words, the parasitic relation is far more specialized than the chief pathogenic product.

3. No strictly invasive bacteria have yet been found producing diffusible toxins which appear to be of any real significance in the disease process.

4. Those which produce such toxins are not strictly invasive bacteria.

5. The injury due to invasive bacteria is known to be due to the disintegration of bacteria and the setting free of poisons locked up in the bodies of the microbes.

6. Pathogenic bacteria manifest less biochemic activity than the related saprophytic forms.

7. The hemolytic and leukocidic toxins of bacterial filtrates may be due to autolysis of the bacteria. Jordan has shown that hemolysis is at least in part due to a change in the reaction of the culture fluid.

According to this hypothesis, microorganisms in slowly adapting themselves to the parasitic habit would gradually eliminate active toxin production and other aggressive weapons as of little use, and strengthen whatever defensive mechanisms they may accidentally possess the rudiments of. If these are capable of marked development, we may expect such types of disease as tuberculosis, leprosy, glanders and syphilis, in which the parasitic habit is carried to a high state of perfection. If their mechanisms of defense are not capable of much development they will soon be destroyed or else become adapted to live upon the skin, and especially the mucous membrane as opportunists and occasional disease producers.

In this adaptation the possession of somatic poisons set free during disintegration may play an important part. They may give rise to just sufficient toxin to produce a local protecting nidus of necrotic tissue, until the time for escape to some other host arrives. This assumption is supported by the fact that diseases of some duration are usually focal in character. The microorganisms multiply only in certain foci which sooner or later become evident as the visible seat of disease.

It may be claimed that defensive and offensive methods are practically the same, and that it is simply a play upon words to make any distinction between them. But reflection will convince us that offensive methods mean direct injury, whereas defensive methods simply mean a neutralization of the offensive weapons or else a condition which is invulnerable to them, such as an envelope made of a special substance.

According to Ehrlich and his pupils, the antibodies which appear in the course of disease are not new bodies, but overproductions of bodies present in minute quantities normally. The parasitic microbe is thus at the very beginning of the invasion confronted with these bodies. At the

termination of the disease there are no new bodies present, but the antibodies are on hand in relative abundance. The situation which the invader has to face is thus qualitatively the same at the beginning and at the end of the attack. How does he meet it by defensive methods?

Three possible fates await the invaders: (1) They are largely destroyed within the body; (2) they are excreted, or discharged through various channels; (3) they remain indefinitely in the body after the disease is over, to be eventually destroyed or eliminated.

That the microorganisms are largely destroyed within the body in the course of the disease is not open to dispute; this class is of no special significance to us. Of most importance are those that escape to continue their life cycle in another subject. The mechanism of elimination is of vital importance to the parasite. It assumes many forms, and is admirably adapted in the various specific diseases to perpetuate the existence of the species.

The survival of the microbes after the disease is over may be explained partly on the ground that in nearly all diseases some of the microbes pass their final stage near the surface of the skin, or mucous membrane, or in organs in direct or in indirect contact with the outer air, so that escape outward is readily effected through destruction of tissue, and hence protection from the bactericidal forces of living tissue. The small number which in some types of disease remain alive for some time after the disease process has subsided, may also be enclosed in small foci of necrotic tissue, and thus escape destruction temporarily.

I am inclined to believe, however, that among the problems of the future will be the elucidation of still another mechanism for the protection and escape of the microorganism. It is highly probable that in a certain number of species of bacteria after the active vegetative stage a latent stage follows, during which the parasite which has escaped destruction provides itself with some protective envelope which also aids it in its passage to a new host. This envelope, which may be some specific substance not recognizable with the microscope, or which may be represented by the capsules in some groups, may be a defensive body of the parasite stimulated to overproduction by the antibodies of the host. This body also interferes with the metabolism of the microbe and thus acts in the double capacity of stopping the disease and protecting the microbe at the same time. This hypothesis suggested itself to me while endeavoring to account for the peculiar behavior of tubercle bacilli under cultivation.

It is well known that tubercle bacilli from the diseased tissues of cattle grow very slowly, and then only upon certain culture mediums, such as blood-serum. After several years of continuous cultivation they multiply vigorously in glycerin bouillon and can hardly be distinguished in appearance from those human varieties of the bacillus which grow richly from the first or second transfer. There seemed to be no justification to assume that the bacillus had completely changed its metabolism under artificial cultivation. The more rational hypothesis seemed to be the one which assumed the existence of some protective substance only slightly acted upon by the serum, not at all in glycerin bouillon, and therefore a hindrance to multiplication. After repeated transfers, this protective substance was slowly lost either through a selection of bacilli or absence of stimulation on the part of the host, or both causes combined, and the growth became as luxuriant as with the more saprophytic human varieties. It is obvious that each group or species of bacteria would have its

own special protective device depending upon original capacities of the species which would be gradually developed in power and efficiency with the perfection of parasitic relations.

The formation of protective or defensive coverings, the strengthening or modification of the cell wall or the secretion of defensive fluids, would account for certain phenomena which are familiar to bacteriologists much better than the current theory which bases parasitism exclusively upon toxin production, active or passive.

In cultures we should expect a loss of power to form protective substances because the antibodies are absent. Hence the universal tendency toward the reduction and final loss of virulence, with apparently the metabolic and vegetative activities unchanged, and the frequently observed regaining of virulence by passages through series of animals.

In the evolution of parasitic bacteria I assume then that though the function of toxin production may have been the entering wedge toward a parasitic existence, there is a progressive loss of this function as of little use to the parasite after it has once acquired a foothold, for the action of toxins at a distance from the focus of multiplication does not aid the parasite, while it may destroy the host. In other words, with the invasion of the tissues of the latter it became necessary for the invader to concentrate its powers in its immediate vicinity, and for this purpose those poisons set free by the disintegration of the parasite may be of use in protecting the focus where the younger forms are, by necrosis of tissue, plugging of vessels, etc., and thereby keeping away the bactericidal forces until the bacteria have accumulated sufficient protective power to subsist in a latent condition and are ready to be discharged outward. With the loss of active toxin production according to this hypothesis and the loss of other, now useless, metabolic activities, there goes hand in hand a strengthening of the defensive functions. This strengthening I interpret as the gradual development of certain substances which the non-immune host is unable to act upon or at most only in a slight degree. This substance which, as it were, shoves itself between the parasite and the common bactericidal forces of the body, bears the specific pathogenic character of the microbe. It is the substance which, according to the nomenclature of Ehrlich, calls forth the amboceptor from the resources of the host to combine with it, and thus open the way for the usual bactericidal forces or complements according to Ehrlich. The existence of this specific protective body will account for the varied resistance of animals to the same microorganism and the relative difficulty to induce immunity. The more difficulty the body has in producing the amboceptor the greater the difficulty in acquiring immunity.

In the departments of preventive and therapeutic medicine, the isolation of this protective substance apart from the body toxins would be of prime importance in combating disease by inducing individual resistance. In fact, the theory that the so-called immunizing and disease-producing substances are separate is not new, but has been presented under various forms. The tendency to give up the toxic extracts of bacteria and use the latter in their entirety in immunization pays tribute to these unknown bodies. The most prominent example of this change was the abandonment by Koch of the old tuberculin, a boiled extract, and the utilization of the entire tubercle bacilli ground and uninjured by heat, in the production of immunity in tuberculosis.

The foregoing hypothesis, that the tendency of microbes in perfecting the parasitic habit is to act solely on the defensive, is to

a certain degree supported by a phenomenon of considerable biologic importance, which I wish to discuss very briefly.

If we examine the statistics of the various infectious diseases we are struck with the relatively low mortality of most of them. There are few highly fatal plagues now known. To be sure, the mortality of many infectious diseases is regarded as formidable by sanitarians, but if we disengage ourselves from the humane view for the moment and take the biologic standpoint, we will agree that the relatively high mortality of 25 per cent. to 50 per cent. indicates a very decided preponderance of the resisting powers of the human race. The odds are always against the invading microbe. This state of affairs appears for the moment to contradict the results of experimental bacteriology, which teach us that the virulence of microbes may be more or less rapidly raised by repeated passages through susceptible animals, or even through those which possess considerable resistance. The accustoming of bacteria to antiseptics, bactericidal and agglutinative serums, has already been mentioned. With this capacity for adapting themselves to the defensive mechanisms of the host, why should not the infectious diseases become more, rather than less, virulent? What is it that keeps their virulence on a low level? This problem has occupied my attention for a number of years, but only recently did a fairly satisfactory explanation present itself. Before entering upon it I have still one other phase of the problem to consider.

Of a given number of races of the same species of bacteria, the one which becomes parasitic on a given host species is not necessarily the most virulent for that species. This phenomenon impressed itself upon me during the study of a number of races of the bacillus of septicemia hemorrhagica, or, more familiarly, rabbit septicemia. Races of this species have been found very widely distributed among mammals and birds. Epizootics due to it have been described as occurring among cattle, carabao, game, swine, rabbits, guinea-pigs, fowls, geese, etc. It lives in the upper air passages of many domestic animals in health.

The rabbit may be successfully inoculated with any of these races. Some are very virulent, for the merest scratch of the skin inoculated with them will result in death within twenty-four hours. But the rabbit is not attacked spontaneously by them, although they are ubiquitous. The race which has fastened itself upon the rabbit is one of a very low degree of virulence for that animal. Similarly the highly virulent tubercle bacillus of cattle is encountered only occasionally in man, although the opportunities for a transfer from cattle to man are very good.

On first thought, it would seem to us that the most virulent race would be the one to crowd out any less virulent races and to finally predominate. But comparative pathology shows us that the contrary may be true.

The explanation for these apparently discordant facts readily flows from a consideration of the life history of parasitic microorganisms. This briefly consists of three phases, the entry into the host, the temporary multiplication therein, and lastly, the escape to another host. Each step in this life cycle must be carefully and deliberately worked out in the evolution of parasitic organisms, and each demands a special mechanism adapted to the purpose. One step is as important as the other. The parasite must find an unguarded entry or one which yields readily to its efforts. It must have a means of defense within the body and it must finally reach the exterior to enter a fresh subject.

As a result of these needs, each microorganism producing disease has one or sev-

eral avenues of entry and escape. In some of the protozoa there is but one avenue, and this is highly specialized and is through the body of some insect. Among the bacteria the channels of escape are less highly developed, and there may be several. As a rule, the microbe adapts itself eventually to a locus more or less in direct contact with the exterior, and in some instances the loci of entry, multiplication and exit have coincided. If we think over the various infectious diseases of man and animals, of which we have any definite information, we shall be surprised to find in how many the points of attack are in organs or tissues in direct communication with the exterior. In the most common type of tuberculosis, pulmonary consumption, the process is almost wholly limited to the respiratory organs. In typhoid fever the process is to a large degree carried on in the intestinal wall. In dysentery and cholera it is wholly so. Even in the very protracted disease of leprosy, the skin is the chief seat of the disease, while the discharge of bacilli from the ulcers of the nose is the rule in the tuberculous type. In that exquisitely parasitic, highly specialized group of microorganisms producing the eruptive diseases the final process is carried on in the skin. In these diseases the mechanism of escape is the most perfect.

On the other hand, among the spore-bearing pathogenic bacteria the means of escape is uncertain. Thus the anthrax bacillus betrays its saprophytic nature, as pointed out by Koch many years ago, in its inability to produce spores within the body. Were it not for the accidental discharges of blood from the mucous surfaces and the operations of man, the bacillus might not escape at all to sporulate. Similar conditions obtain for the bacillus of tetanus and of Rauschbrand. Both produce disease probably in an accidental manner and not as progressive parasites. Their continued existence is assured by vegetation and spore formation outside of the body, and it is highly probable that the species would continue to exist if they did not attack animal life, and that their incursions into the body are of no use to them. On the other hand, all attempts to demonstrate the production of spores in bacteria whose existence depends largely or wholly upon parasitism have thus far failed. The spore is evidently poorly fitted to parasitism and is replaced by other devices of more adaptability.

The mechanisms of invasion and escape bear a distinct relation to the pathogenic power or virulence. It is safe to assume that those varieties or species, no matter how virulent, will be eventually destroyed if these mechanisms are imperfect. In fact the more virulent the microbe, the more rapid the death as a result of invasion, the less the opportunity for escape. Hence there will be a selection in favor of those varieties which vegetate whence they can escape. The surviving varieties would gradually lose their highly virulent invasive qualities and adapt themselves more particularly to the conditions surrounding invasion and escape. That some such process of selection has been going on in the past seems the simplest explanation of the relatively low mortality of infectious diseases. These individuals or races of microbes which invaded the host too rapidly and caused death would be destroyed in favor of those which vegetated more slowly and in tissues permitting escape of the microbe after a certain time.

We may now return to the rabbit septicemia bacillus. The reason why the most virulent type of this group does not pass to rabbits is due to the fact that there is no satisfactory mechanism of entry and escape. This presupposes a lesion, a wound as a place of entry, and the excretion and

transfer into a wound in another animal. In the rabbit this difficulty is worked out in the way usual with this bacillus. The microbe adapts itself to vegetate upon the mucous membrane of the upper air passages. Under certain conditions it invades the lungs, pleural and pericardial, more rarely the peritoneal cavity, producing pneumonia and extensive exudates on the serous membranes, and causing death. The disease of the thoracic organs evidently follows some predisposing cause, which enables the bacillus to make a temporary invasion from the mucous membrane. This incursion into the body is not essential to the life of the race. In fact a little reflection will show that the bacteria which invaded are not likely to be transmitted, whereas those on the mucosa are readily handed down from old to young. The virulence of the bacillus is thus kept on a low level, so low that subcutaneous inoculation of pure cultures produces merely a local lesion. This type of disease is quite different from that produced by inoculation with highly virulent races. These multiply rapidly in the blood throughout the body.

We can now appreciate Pasteur's failure to exterminate the rabbits of Australia. He believed that with races of this bacillus on hand which destroy life very quickly, all that is necessary is to start the disease among rabbits, and it will tend to spread. The stricken rabbit with its blood full of germs does not offer the means for inoculating a second, and so the virulent race perishes.

We can understand, furthermore, why the bacteria associated with definite diseases in animals produce a diseased condition with difficulty after inoculation. The virulence of the specifically adapted microbe is of a relatively low order, and in the production of epizootics various conditions must be realized which assist the microorganism. The careful analysis of these conditions will form one of the great problems of pathology in the immediate future.

The phenomenon of the elimination of the most virulent races and the establishment of parasitic races of less invasive power I have portrayed in the simplest terms. But it is probably much more complex. The parasite, to be successful, must also stand in a definite relation to the tissue through which it enters. It is quite probable that the race of rabbit septicemia bacilli of high virulence would not be able to maintain itself in the mucus of the upper air passages. This ability to multiply in certain places is evidently an acquisition which gives the particular race its specific character. Without doubt the bovine tubercle bacillus, though of great virulence, does not possess the specific power of entering the human body, or it may be of maintaining itself after entry in certain tissues, such as the lymph-nodes, except under certain accidental, favoring conditions not yet understood. Perhaps the process of cultivating vaccine virus in the skin has deprived it of the capacity for entering through the respiratory tract, and has converted it into a purely inoculable virus.

In the study of pathogenic bacteria the relative ease with which pure cultures may be obtained from the blood and other organs only accessible by way of the blood has made this a favorite way of obtaining such cultures. But it may be asked whether we are not in this way obtaining bacteria of maximum virulence. May not the non-agglutinability of some typhoid bacilli immediately after isolation be accounted for in this way? In general, the bacteria thus obtained can differ but little from the type, as they are all recently descended from a single bacillus or a very few which caused the infection. It is different in the so-called passages through series of animals in which the usual portals

of entry and exit are circumvented and the bacteria injected into the body and withdrawn therefrom directly. As a result of such passages many species of bacteria have been made more virulent, and Pasteur was able to greatly modify the unknown virus of rabies.

Besides the maintenance of virulence and its occasional augmentation, a slow decline to complete loss of virulence may be looked for under conditions abnormal for the microbe. This probably goes on where the bacteria multiply, partly or wholly protected from bactericidal influences. The bacilli of tuberculosis, which multiply in cavities of the lungs or in muco-pus of the air-tubes in chronic cases, must be regarded as degenerating in virulence. And we actually encounter races varying considerably in pathogenic power. In the throats of well persons or those who had diphtheria months ago, bacilli without any power of toxin production, but with all the other characters of genuine diphtheria bacilli, are occasionally encountered.

During the elimination of the more virulent races of microorganisms, there goes on as well a gradual weeding out of the most susceptible hosts. In a state of nature in which medical science plays no part, there must occur a slight rise in the resistance of individuals, due to selection and perhaps acquired immunity, which meets the decline of virulence on the part of microbes until a certain norm or equilibrium between the two has been established. This equilibrium is different for every different species of microorganism, and is disturbed by any changes affecting the condition of the host or the means of transmission of the parasite. One result of the operation of this law is the low mortality of endemic as compared with epidemic diseases. Certain animal diseases, while confined to the enzootic territory, cause only occasional, sporadic disease, but as soon as they are carried beyond this territory, epizootics of high mortality may result. Climate in some cases enters as an important factor, but the most important, perhaps, is the slight elevation in virulence brought about by a more highly resistant host. The most susceptible animals are weeded out, and the rest strengthened by non-fatal attacks. The virulence of the microbe rises slightly to maintain the equilibrium. In passing into a hitherto unmolested territory, the disease rises to the level of an epizootic until an equilibrium has been established.

The same is true of human diseases, among which smallpox is a conspicuous example. The great pandemics of influenza, which seem to travel from east to west every one or two decades, soon give away to sporadic cases, and the careful work of many bacteriologists would indicate that the influenza bacilli found at present have fallen to the level of secondary invaders, and are parasites of the respiratory tract in many affections.

As pathogenic microorganisms differ not only in the degree of parasitism attained, but also in their essential nature, a great variety of diseases is the result. In a crude way they may be arranged into three classes:

1. Microorganisms which live upon the skin and the mucous membranes and invade the body only when lesions exist in these structures, or where the general resistance is impaired.

2. Microorganisms which appear only occasionally from some unknown but permanent focus. They produce epidemics often highly fatal, but they are successfully pushed back because the strain can not readily adapt itself to the new conditions.

3. Microorganisms which are most highly adapted for a parasitic existence and which produce diseases of a relatively fixed type.

As regards the first class, the conditions under which they produce disease rise more and more into prominence. The factor microbe becomes almost secondary to other factors. Many of our most common diseases obey certain meteorologic laws. Thus diphtheria and pneumonia are chiefly winter diseases, because the conditions of throat and lungs which favor them are largely due to cold weather, or we might say, the cold weather acting upon an indoor sedentary population or one subjected to untoward influences, injures the respiratory tract. Some microbes of this class depend upon the preparation made for them by others. Thus the exanthematous diseases, such as scarlatina and smallpox, are frequently associated with or followed by the invasion of streptococci, and the majority of deaths are due to such secondary invasion. The streptococci live upon the mucous membranes, and whenever the proper opportunity comes they invade more vital territory. This group of bacteria is the frequent cause of death in chronic diseases. Some years ago Professor Flexner pointed this out and denominated the invasion as a terminal infection. I think that they may also be appropriately styled the parasites of the diseased state.

Among the second group we may place such diseases as Asiatic cholera and the bubonic plague. The origin of the first is unknown. The definitive host of the second is probably the rat.

Among the third class we have such groups of diseases as tuberculosis, leprosy, syphilis and glanders, on the one hand, and the eruptive diseases, on the other. The former are very chronic, protracted, the widely separated but highly parasitic latter acute, rapid in their course. In the eruptive diseases the infection seems to depend solely upon the specific susceptibility of the individual, and immunity is easily brought about by protective inoculation.

In tuberculosis and leprosy the mode of infection is evidently very different from that of the group just mentioned. Prolonged exposure, as in family life, seems necessary to successful infection, and even then many exposed individuals escape. In tuberculosis, heredity plays a very prominent part in the eyes of the physician, because the disease appeared to propagate itself in families. This was probably due to the necessity for more intimate association and repeated exposure in order that the disease might appear. Here the disease is long drawn out, the parasite may become in a sense individualized, and the attack upon a new host may have to be made repeatedly. With these highly parasitic forms the necessity for a frequent transfer to another host is slight. In leprosy, the disease may last fifteen years to twenty years, and then death ensues, usually as a result of the attack of the secondary invaders.

From the biologic standpoint which I have endeavored to present, we may conceive of all highly pathogenic bacteria as incompletely adapted parasites, or parasites which have escaped from their customary environment into another in which they are struggling to adapt themselves, and to establish some equilibrium between themselves and their host. The less complete the adaptation, the more virulent the disease produced. The final outcome is a harmless parasitism or some well-established disease of little or no fatality, unless other parasites complicate the invasion. The logical inference to be drawn from the theory of a slowly progressive parasitism would be that in the long run mortality from infectious diseases would be greatly reduced through the operation of natural causes. But morbidity would not be diminished, possibly greatly increased, by the

wider and wider diffusion of these parasites, or potential disease producers. The few still highly mortal plagues would eventually settle down to sporadic infections or else disappear wholly because of adverse conditions to which they can not adapt themselves.

In this mutual adaptation of microorganism to host there is, however, nothing to hinder a rise in virulence in place of the gradual decline if proper conditions exist. In fact, it is not very difficult to furnish adequate explanations for the recrudescence and activities of many diseases today, though the natural tendencies are toward a decline in virulence. In the more or less rapid changes in our environment due to industrial and social movements the natural equilibrium between host and parasite established for a given climate, locality and race or nationality is often seriously disturbed and epidemics of hitherto sporadic diseases result. Typhoid fever will serve as one illustration of my thesis. It is ordinarily a sporadic infection, passing from the sick to the well by direct contact. Our knowledge that the infection of this and other diseases is contained in the discharges of the sick and a growing sense of cleanliness led years ago to the large systems of sewerage, which have made a crowded city life possible. But the removal of sewage from our immediate surroundings was the beginning of new trouble. The sewage was led into water courses from which drinking water came. Hence the great epidemics in place of sporadic disease. The direct transmission of the parasite on a small scale was largely checked, but the indirect transmission greatly favored. The dweller in cities with unprotected water-supply is still further endangered by the fact that the typhoid bacilli returned in the water may represent more virulent varieties than those handed down by his ancestors in rural communities. The motley population brought together by migrations from all parts of the globe bring the various races of bacilli with them to be redistributed on a large scale.

Conditions may even create diseases artificially. Thus in childbirth, the physician through want of cleanliness may in his examination actually inoculate a wounded surface with streptococci or other septic bacteria. In a hospital badly managed, such germs may be made to pass artificially through a series of individuals and their virulence raised. In nature this could not take place, because there would be no physician. Hence the transfer would not take place. The history of maternity hospitals before the period of asepsis in surgery is a sufficient proof for the theory advanced. Hospital erysipelas and hospital gangrene were diseases artificially bred. With the introduction of the principle of asepsis in medicine and surgery the artificially created diseases were destroyed, because the transportation facilities of the bacteria were cut off.

These illustrations indicate that so-called natural law does not stand in the way of our having highly virulent types of disease if we are ignorant enough to cultivate them. The microorganism is sufficiently plastic to shape itself for an upward as well as a downward movement. Among the most formidable of the obstacles toward a steady decline of mortality is the continual movement of individuals and masses from one part of the world to another, whereby the partly adapted parasites become planted as it were into new soil and the original equilibrium destroyed. These various races of disease germs become widely disseminated by so-called germ carriers, and epidemics here and there light up their unseen paths. Fortunately for us, the conditions under which these microorganisms may establish themselves are in

many cases so complex that they can not be realized. It is highly probable that the bubonic plague can not get a foothold or maintain itself among us, while Asiatic cholera might have a better chance, through our still greatly unsatisfactory water-supplies. Many tropical diseases would fail to take root in our climate. The mysterious rise and disappearance of leprosy in the middle ages has astonished many students of epidemiology. Possibly some slight bias of the microorganism may have accomplished what seems almost a miracle. Perhaps the right race or variety once introduced may repeat the history of the Middle Ages in our day or in that of the coming generation.

Another obstacle to the amelioration of infectious diseases is the rapid change going on in the habits of individuals and the ferment in our conceptions of health and well-being, which are continually upsetting any established equilibrium and making us more resistant to some diseases, more susceptible to others. Of great interest is the effect upon the human race of the assiduous care of those afflicted with certain chronic diseases which is just now expressing itself in the establishment of sanatoriums for the cure of the tuberculous. If this movement should gain great headway there may be a race of immunes gradually developed who may be able to stand the untoward conditions of indoor city life much better than the naturally robust and physically superior who have no so-called hereditary taint.

Of still greater interest is the vast vaccination experiment to whose beneficent influence the century just past bears ample testimony. The vaccinated individual is either wholly immune or else the disease contracted after exposure is abortive and the eruptive stage does not come to full development or maturity. The excretion of the infecting organism is thereby greatly interfered with and it is not improbable that in the mildest cases it may not reach that maturity necessary for the successful infection of others. In view of the adaptability of microorganisms in general, it is not beyond the range of possibility that a variety of the smallpox organism may through a chain of accidents arise as a result of successive passages through partly protected individuals. To-day it seems fairly well established that a single vaccination in infancy is not an adequate protection during life and at least one nation —a nation which not only cultivates but consistently utilizes science—prescribes two vaccinations as necessary to complete protection. Whether in the days of Jenner repeated vaccinations were deemed necessary I have not been able to verify; but we may assume without immediate fear of experimental contradiction that a century of incomplete protection may have worked some changes in the smallpox organism. In any case, it is obvious that our thesis implies in addition to the natural decline of virulence also a gradual rise in virulence whenever the resistance of susceptible individuals is raised on a very large scale. Either the microorganism if a true parasite will perish or else it will augment its invasive powers to meet those of its host.

Another problem has been created for the diphtheria bacillus by the extensive use of diphtheria antitoxin. Will the thorough protection of one group of human beings lead to the decline or to the increase in virulence of the diphtheria bacillus circulating among the individuals of this group? What effect will the transfer of such bacilli to unprotected groups have? These and similar queries may be answered not many years hence, for a generation of microbes represents a very short space of time.

It may not be out of place to call atten-

tion here to the bearing of my thesis upon the recent attempts to utilize parasitism in ridding us of undesirable or noxious animals. In bacteriology there have been attempts to destroy field mice and rats with certain species of bacteria. In entomology, parasitism is such a familiar phenomenon that it has been seized upon on a number of occasions to destroy otherwise unassailable insect pests.

Leaving out of consideration the presumptive dangers of introducing new species into a locality or country which must always be taken into consideration, although they may be of no significance, we have to consider the chances of success as compared with the cost of introducing and maintaining the parasites. In any event, we need not expect a destruction of the noxious species, for that is not the end of parasitism. A reduction in numbers is all that need be looked for. The new parasite will probably fail to become acclimated at first, and it may be necessary to reintroduce it for a number of years. During this period some few may become adapted to their environment and continue as parasites. Whether the equilibrium finally established will be of economic value, must be observed rather than predicted. In bacteriologic experiments of this kind the continued vigorous activity of the bacteria from year to year need hardly be expected. The disease will either die out or continue on a low level of mortality, in accordance with the general laws I have detailed, unless bacteria whose destructive powers are maintained and carefully gauged in the laboratory are distributed at definite intervals.

In conclusion, I will simply call attention to another problem affecting the future well-being of mankind, the possibility of new infectious diseases arising in the flux and change incidental to human progress. We have assumed that the capacity for a parasitic existence probably depends on some original offensive power of the microbe which it accidentally possessed, such as toxin production, or the presence of intracellular toxins combined with defensive powers. These, possessed independently of the host, were probably the entering wedges to be further developed or dropped, according to necessity. It is more than probable that all species of bacteria which possess these rudimentary invasive powers have already availed themselves of the opportunity to become parasites of animal life, on the one hand, of vegetable life, on the other, and that no startlingly new diseases will arise from saprophytic forms.

Subsidiary problems there are, however, concerning the modifications and readaptations of the parasitic forms already in existence. These may be grouped under two heads:

1. The transfer and adaptation of parasites from one host species to another.

2. The increase of invasive properties of parasites of the same host.

Are there any new diseases likely to appear as a result of the successful adaptation of parasites of higher animals to the human subject? This is a legitimate question, though difficult to discuss, for want of material at present. Among the more important possibilities I will simply mention the bovine tubercle bacillus and the hog cholera group of bacteria. The larger number of parasites on animals are so specialized, however, their receptor apparatus, according to Ehrlich, may have been so curtailed that parasitism on a relatively distant species may be impossible.

As regards the second problem, that microbes may gain in invasive power on the same host, the principle I have endeavored to establish would stand in the way of any rise in virulence because the most invasive forms of a varying species would have the

least chance for transmission. Whatever increase in disease-producing power may be acquired must be gained under special conditions, one of which is association with other microbes. Thus, if we could conceive of the same streptococcus, originally an inhabitant of the normal throat, as passing on account of some series of accidents through the bodies of a number of scarlatina patients, this streptococcus might thereby rise temporarily to the level of a serious menace to the throats and perhaps other organs of relatively healthy people.

Again, certain microbes like *B. coli*, the pneumococcus and meningococcus may, by living upon catarrhal mucous membranes and passed by case to case, acquire enough temporary pathogenic power to cause localized epidemics under favorable conditions. Any advantage thus gained would soon be sacrificed and the microbe return to the normal condition unless a satisfactory mechanism of transmission could be established.

It will be seen that there are many problems before the bacteriologist, problems which have something akin to those of the student of races, varieties and species among higher forms of life. These problems must be attacked with the same patience and pertinacity that were exercised by Mendel, Darwin, De Vries and many others in the effort to trace the rise of new species.

In dealing with the great problems of pathogenesis and parasitism as applied to the microorganisms in such a summary and hasty manner, and in endeavoring to trace the law of a declining virulence (and hence mortality) and an advancing parasitism, I may have left some doubts in the reader's mind concerning the ultimate value of medicine, preventive and curative, in controlling these diseases, since it might be assumed, according to the hypotheses presented, that they would take care of themselves. This impression will, I think, be dispelled by a little further development of the ideas presented.

The social and industrial development of the human race is continually leading to disturbances of equilibrium in nature, one of whose direct or indirect manifestations is augmentation of disease. In order to avoid this calamity or reduce its force as much as possible we must make special compensations or sacrifices to restore or maintain the normal balance. The more clearly the kind of compensatory action required is foreseen, the more promptly it is put into effect, the less disease there will be. It is the true function of medical science to discover and put into effect those compensatory movements which will counterbalance the temporary ill effects of what, for want of a more illuminating term, we call human progress.

It is largely through the phenomenon of parasitism that nature attempts to restore the equilibrium, and in this microorganisms play the most important part. As soon as the individual falls below a certain level he may become the prey of a microscopic, or even an ultramicroscopic, world. Hence the importance of bacteriology in medical science. Much has already been done in determining ways and means for the counterbalancing of the ravages of this microscopic world, but science can not rise above natural law, but must work through it. The optimism of the world frequently places science above natural law and believes it capable of correcting any and all excesses of individuals and races We may be certain that it will never be able to eliminate the factor of parasitism. Its most important work will continue to be to analyze this factor into its minutest details and to devise means by which this analysis may be made useful in turning aside or at least in deadening the shock of disease.

Theobald Smith.

Medical School of Harvard University.

SCIENTIFIC BOOKS.

POINCARÉ'S 'SCIENCE AND HYPOTHESIS.'

La Science et l'Hypothèse. Par H. POINCARÉ, Membre de l'Institut. Paris, 1903. Pp. 284.

Wissenschaft und Hypothese. H. POINCARÉ. Autorisierte deutsche Ausgabe, mit erläuterenden Anmerkungen, von F. und L. LINDEMANN. Leipzig, 1904. Pp. xvi + 342; the notes, pp. 245–333.

A work from the pen of one of the distinguished savants who have so recently been the guests of the American scientific public is doubly interesting at the present time. Among the several domains of pure and applied mathematics which M. Poincaré has enriched by his researches, not the least important is that of the fundamental concepts and logical development of various branches of science. Like its predecessors, the work under consideration here is remarkable for the clear, incisive and succinct fashion in which it deals with the difficult and elusive problems lying at the foundation of mathematical knowledge.

The work is divided into four parts, preceded by a short introduction, *viz.:* First Part: 'Number and Magnitude,' pp. 9–48. Second Part: 'Space,' pp. 49–109. Third Part: 'Force,' pp. 110–166. Fourth Part: 'Nature,' pp. 167–281.

The first chapter is entitled, 'On the Nature of the Reasoning of Mathematics.' At the very outset, even the existence of the science of mathematics seems to present an irreconcilable contradiction. If mathematics is deductive, drawing all its conclusions strictly from their antecedent premises, how can it be more than a huge tautology? How are all the ponderous tomes of mathematical theory aught else than devious ways of saying *A* is *A*? If, on the other hand, the conclusions of mathematics say more than their antecedent premises, how is the unquestioned perfect rigor of mathematics maintained?

M. Poincaré finds the answer to these questions in the so-called 'mathematical induction' which proceeds from the particular to the more general, but at the same time does so by steps of the highest degree of certitude. In this process he sees the creative force of mathematics, which leads to real proofs and not mere sterile verifications. The illustrations used to make the thought clear are taken from the beginnings of arithmetic, where mathematical thought has remained least elaborated and uncomplicated by the difficult questions related to the notion of space. In successive instances it is shown how more general results are obtained from fundamental definitions and from previous results by means of mathematical induction. In each case the advance is made by virtue of that "power of the mind which knows that it can conceive of the indefinite repetition of the same act as soon as this act is at all possible. The mind has a direct intuition of this power and experience gives only the opportunity to use it and to become conscious of it" (pp. 23–4).

The conviction that the method of mathematical induction is valid our author regards as truly an *à priori* synthetic judgment; the mind can not tolerate nor conceive its contradictory and could not even draw any theoretic consequences from the assumption of the contradictory. No arithmetic could be built up, rejecting the axiom of mathematical induction, as the non-Euclidean geometries have been built up, rejecting the postulate of Euclid.

The second chapter terminates the first part and is entitled, 'Mathematical Magnitude and Experience.' It deals with irrational numbers and the creation of the mathematical continuum, concluding that 'this notion has been created by the mind, but that experience furnished the occasion' (p. 35). "The mind has the power of creating symbols, and by this means it has constructed the mathematical continuum which is merely a particular system of symbols. This power is limited only by the necessity of avoiding contradiction, but the mind makes use of it only when experience furnishes the warrant" (p. 40).

The second part, devoted to 'Space,' consists of chapters on 'The non-Euclidean Geometries,' 'Space and Geometry' and 'Experience and Geometry.'

In this part the fundamental question is: What is the nature of the axioms of geometry? Our author's views may be seen in the following quotations:

The axioms of geometry are neither synthetic judgments à priori, *nor experimental facts.* They are *conventions:* our choice among all possible conventions is *guided* by experimental facts, but it remains *free* and is limited only by the necessity of avoiding all contradiction. Hence the postulates can remain *rigorously* true even though the experimental laws which have determined their adoption are only approximative.

In other words, *the axioms of geometry* (I am not speaking of those of arithmetic) *are merely disguised definitions.* Consequently the question: 'Is Euclidean geometry true?' has no meaning. As well ask whether the metric system is true and the old measures false, whether Cartesian coordinates are true and polar coordinates false. One geometry can not be more true than another, it can only be *more convenient.*

Euclidean geometry is and will remain the most convenient:

1. Because it is the simplest; and it is so not only in consequence of our mental habits, or of I know not what direct intuition we may have of Euclidean space, but it is the simplest in itself, just as a polynomial of the first degree is simpler than one of the second.

2. Because it accords well with the properties of natural solids.

Beings with minds and senses like ours, but who had received no previous education, might receive, from an external world suitably chosen, impressions such that they would be led to construct a geometry other than that of Euclid and to localize the phenomena of that external world in a non-Euclidean space, or even in a space of four dimensions.

If, on the other hand, we whose education has been received in our actual world were suddenly transported into this new world, we should have no difficulty in relating its phenomena to our Euclidean space (pp. 66–8).

If the geometry of Lobatscheffsky is true, the parallax of a very distant star would be finite; if that of Riemann is true, it would be negative. These are results which seem within the reach of experiment, and there have been hopes that astronomical observations might enable us to decide between the three geometries.

But in astronomy 'straight line' means simply 'path of a luminous ray.' If, to suppose the impossible, negative parallaxes were found, or if it were demonstrated that all parallaxes are superior to a certain limit, two courses would be open; either we could renounce Euclidean geometry, or we could modify the laws of optics and admit that light does not travel rigorously in a straight line. It is useless to add, that every one would regard the latter as the more advantageous, Euclidean geometry has nothing to fear from new experiments (p. 93). "No experience will ever contradict the postulate of Euclid, nor will any ever contradict that of Lobatscheffsky" (p. 95).

The third part, devoted to force, consists of chapters dealing with 'Classic mechanics,' 'Relative movement and absolute movement' and 'Energy and thermodynamics.'

Here, as in geometry, our author finds that the fundamental principles are neither *à priori* truths nor experimental facts but convenient definitions or conventions.

If the principle of inertia, for example, were an *à priori* truth, how could the Greeks believe that movement ceases as soon as the cause which originated it ceases to act? How could they believe that every body free from constraint would move in a circle, the noblest of all motions?

Is there any more warrant to say that the velocity of a body can not change without cause for the change, than that it can not change its position or the curvature of its path except under the influence of an exterior cause?

Have any experiments ever been made on a body subject to no force, and if so how was it known that no force was acting? A sphere rolling on a marble table for a very long time is a usual example, but has the force of gravity ceased to act?

Can the law that *the acceleration of a body equals the force acting on it divided by its mass* be verified experimentally? To do so the acceleration, the force and the mass must be measured. If we overlook the difficulties connected with the measurement of time, it may be granted that the acceleration can be measured, but there are inextricable difficulties in the definition of *mass* and *force.* Useful definitions must teach how to *measure* mass and force, and require definition of the

equality of two forces, and this implies the principle of the equality of action and reaction. "Hence, this principle should no longer be regarded as an experimental law, but as a definition" (p. 122). The result reached is that the 'law of Newton' as to acceleration must be regarded as a definition, in which *mass* is still undefined. "We are driven to the following definition, which is simply an avowal of impotence: *Masses are coefficients which it is convenient to introduce into calculations*" (p. 127).

While the principles of dynamics are definitions, they can be approximately verified by experiment. A more precise experiment would show simply that the law was only approximately true in that case; which we knew already. Thus we see how experience has served as basis for the principles of mechanics and still can never contradict it.

The analogy between geometry and mechanics would at first glance seem complete. In each the fundamental principles are merely conventions which experience has led us to set up as convenient. But there is a difference. The laws of geometry are set up in consequence of experiments in mechanics, in optics, in physiology; they are in no sense experiments in geometry; they do not relate to space (which geometry studies), but to material objects. On the other hand, the fundamental conventions of mechanics and the experiences which show that they are convenient, relate to the very same objects or to analogous objects. This is not an artificial barrier between sciences but a real distinction. The teaching of mechanics should, therefore, remain objective, experimental.

The fourth part, devoted to 'Force,' contains chapters on: 'Hypotheses in physics'; 'The theories of modern physics'; 'The theory of probabilities, optics and electricity,' and 'Thermodynamics.' In this part the relation of observation to hypotheses and generalization is taken up. Experience is the sole source of truth, but one must use his observations; he must generalize. A mere accumulation of facts is no more a science than a pile of stones is a house. Above all, the scientist must foresee. A good experiment teaches more than an isolated fact; it permits us to foresee, *i. e.*, it permits us to generalize. Interpolation is necessary. Experiments give us only a certain number of isolated points; generalization traces a curve. This curve does not pass exactly through all the points given by experiment. We not merely generalize experience, but correct it. Experimental physics furnishes the facts; mathematical physics orders them, makes the generalizations and points out the needs. In this generalization the unity of nature and the simplicity of its laws is presupposed. The curve does not follow all the zigzags indicated by the points given by experiment. Nevertheless, it is not certain that nature is simple, but generalization, and with it science, could not exist if the hypothesis of simplicity were entirely abandoned.

Generalization requires hypotheses. There are three categories of hypothesis: (1) Those which are natural and which can hardly be avoided, as that the influence of very distant bodies is negligible; (2) those that are indifferent, as that matter is continuous or that it is composed of atoms. These indifferent hypotheses are never dangerous, provided their true character is recognized. The hypotheses of the *third* category are true generalizations which experience should either confirm or invalidate.

The hypotheses of physics lead to physical theories which, though apparently well established, are in turn displaced by others. Various examples are discussed.

> "No theory seemed more solid than that of Fresnel which attributed light to movements of ether. But now that of Maxwell is preferred. Does this mean that the work of Fresnel was in vain? No, because the real aim of Fresnel was not to find out whether there really is ether, whether it is or is not formed of atoms, whether these atoms really move in this or that sense; his object was to foresee optical phenomena.
>
> Now the theory of Fresnel always permits this, to-day as well as before Maxwell. The differential equations are always true; they can always be integrated by the same procedure and the results always retain their value.
>
> Let no one say that thus we reduce physical

theories to the rôle of mere practical recipes; these equations express relations, and if the equations remain true it is because these relations preserve their reality. They teach us, now as then, that there is such a relation between such a thing and such another thing; only this something which formerly we called *movement* we now call *electric current*. But these appellations were only images substituted for the real objects which nature will eternally hide from us. The veritable relations between these real objects are the only reality that we can attain, and the only condition is that the same relations exist between the objects as between the images by which we are forced to replace them. If these relations are known to us, what matter if we deem it convenient to replace one image by another.

That some periodic phenomena (an electric oscillation, for example) is really due to the vibration of some atom which, acting like a pendulum really moves in this or that sense, is neither certain nor interesting. But that between electric oscillation, the movement of the pendulum and all periodic phenomena there exists a close relationship which corresponds to a profound reality; that this relationship, this similitude or rather this parallelism extends into details; that it is a consequence of more general principles, that of energy and that of least action, this is what we can affirm; this is the truth which will always remain the same under all the garbs in which we may deem it useful to deck it out" (pp. 189–191).

Our author has thus discussed the question of the degree of reality in various branches of science from four points of view. In arithmetic we have necessary truth developed *à priori* in the mind; in geometry we have to do with conventions, conveniently related to the material world, but not themselves amenable to direct experimental treatment; in mechanics we have likewise to do with conventions, but they are amenable to direct experiments; while in physical sciences we seek under various images to express relations which are profound realities.

It is impossible to give a summary of a work which is itself so summary. What precedes is an inadequate attempt to present a few characteristic views which may serve to indicate the general spirit of the work and the style of treatment. The larger part of the rich mass of material has necessarily remained untouched.

The work is characterized throughout by masterly clearness and by the skill with which the overgrowth of unessentials and consequences is stripped off and the fundamental idea presented in a few phrases. In its tone, the work addresses the non-scientist. Little technical knowledge is requisite to read it, but still it will hardly prove inviting to those who have not in some way attained a certain facility in following strict reasoning. To these it will furnish an excellent and stimulating discussion of some fundamental principles of modern science apart from the technicalities, while the scientist will welcome this presentation in connected form of carefully thought out views which have already aroused much interest in their earlier publication in various journals.

The work is also remarkable for the ease and directness of its style and for the genial manner in which the illustrative examples are chosen and treated. M. Poincaré is a past master of that most difficult art of giving the central thought of a large theory in a few words without sacrificing lucidity.

It is to be hoped that the work will receive in America that wide and thoughtful reading which it deserves equally on account of the subjects treated and the stimulating originality of the treatment. An English translation of the book and of the notes of Lindemann is a desideratum.

Of the German translation little need be said. It is faithful and quite close, and acquits itself remarkably well of the difficult task of conveying the delicate and precise thoughts of the author into the German tongue. The task was of course much facilitated by the remarkable clearness of the original, in which there is seldom opportunity to question just what is meant, though the domain is one where few can avoid involved ideas and entangling phraseology. The imperative requirement that every shade of meaning be faithfully reproduced effectually restrains the translator from any of those paraphrases which must be permitted if the translation is to conform itself, unhampered, to

the genius of the language. In view of these restrictions, the translation seems good, but of course, other things being equal, preference will be given to the original.

A few points of detail may be mentioned: Page 9, lines 3 and 4 should read: '. . . dass er auch für $a = \alpha + 1$ gilt, wenn er für $a = \alpha$ richtig ist.' Lines 8 and 9 analogously.

Page 91, the essential phrase, 'ce qui est expérience, ce qui est raisonnement mathématique' (p. 111 of original) has not been translated.

Page 92, line 2, read 'ist' instead of 'wäre.'

The original, pp. 31 *et seq.*, ascribes to Kronecker that definition of number (as a partition of all rational number into two sets) which is commonly known as Dedekind's. The translation renders all these passages impersonally, and a note calls the presentation of the text Dedekind's, as modified by Tannery.

The notes added to the translation have decided value of their own, and make it desirable either to own both editions or on their account to give the translation the preference. They are to a considerable extent bibliographic, giving excellent lists of references to other works, many of them classic, on the numerous topics which come up. In this respect alone, the notes constitute a welcome and useful supplement to the original work, which makes citations only in the most general way with almost no specific references. But they also develop in many instances mathematical treatment of points touched on in the original, which contains practically no such matter. Frequently the notes state briefly the views of others on the topic in hand, or sketch its historical development, usually with detailed references.

A good index and a fuller table of contents have been added in the German edition.

J. W. A. YOUNG.

THE UNIVERSITY OF CHICAGO,
October 17, 1904.

THE NEW SEISMOLOGY.*

IN the old seismology the only earthquake tremors studied were those of sensible magnitude, and the records related chiefly to destructive effects. The earliest philosophy of the subject regarded the tremor chiefly as a cause, ascribing to it various geologic results, such as the uplifting of coasts and the eruption of volcanoes; and only by slow degrees did it come to be recognized as an effect, the jar communicated by subterranean rending. The new seismology employs instruments of the most delicate and sensitive character, and by their aid not only detects tremors far too faint for direct perception, but undertakes to measure in absolute terms the amplitude, period and speed of the waves and the intensity of the shocks. Its analysis discriminates earth waves of four different kinds, classifies shocks according to origin as volcanic or tectonic, and by means of its data discusses the physical condition of the earth's interior. In a volume recently issued Dutton sets forth the present condition of the science, sketching its history in outline, describing its instruments and characterizing its progress toward the solution of its more important problems. The treatise is well balanced, compact and as comprehensive as consists with adaptation to the needs of the general reader. Technicalities are avoided so far as practicable, and details are introduced only for the purpose of illustrating principles. While it does not neglect that aspect of the subject which falls within the domain of mechanics, and properly gives a major share of space to the treatment of tremors as elastic waves, it is especially strong in its discussion of the bearing of seismology on geophysics. Fortunately for the geologic as well as the general reader, the author brought to his task not only the experience acquired in monographing the Charleston earthquake, but the mental equipment resulting from prolonged study of volcanism and the greater problems of the inner earth.

The discovered blemishes of the book consist of occasional lapses, either of statement or of correlation between text and illustration. For example, the symbol a (page 175), which stands for the intensity of a shock at unit

* 'Earthquakes in the Light of the New Seismology,' by Clarence Edward Dutton, Major U. S. A. [No. 14 of The Science Series.] New York, G. P. Putman's Sons; London, John Murray, 1904.

distance from its origin or centrum, is erroneously defined as 'the intensity at the epicenter'; and the diagram on page 186 indicates the intensity of a shock in the locus technically called the pleistoseist as about four ninths of its intensity at the epicenter, whereas the text shows the ratio to be three fourths. Slips of this character, which might have been eliminated by more careful revision of copy and proof, will doubtless be avoided in subsequent editions. They detract but slightly from the general value of the work, which may be commended to the public as a lucid, attractive, and at the same time scientific presentation of a subject so difficult that its modern aspect is little understood outside the circle of its special students.

G. K. GILBERT.

SCIENTIFIC JOURNALS AND ARTICLES.

The American Naturalist for October has papers on 'The Anatomy of the Coniferales' (concluding article), by D. P. Penhallow; 'Studies of the Plant Cell, IV.,' by B. M. Davis, and 'The Affinities of the Ophioglossaceæ and Marsiliaceæ,' by D. H. Campbell.

The Popular Science Monthly for December contains the following articles: 'The Reclamation Service,' by F. H. Newell (deals with the problems of irrigating the arid regions of the west); 'Chinese and Japanese Immigration,' by Allan McLaughlin; 'The Status of American College Professors,' by John J. Stevenson; 'A Decade of Library Progress in America,' by William W. Bishop; 'Nature's Hieroglyphics,' by Richard S. Lull (treats of the fossil footprints of the Connecticut Valley); 'The Present Problems of Physiological Chemistry,' by R. H. Chittenden; 'The Agricultural Distribution of Immigrants,' by Robert DeC. Ward; 'The Conceptions and Methods of Psychology,' by J. McKeen Cattell. There are also shorter articles and notes on the progress of science, the latter including the two hundredth anniversary of the death of Locke and the two hundred and fiftieth anniversary of Columbia University.

The American Museum Journal for October is devoted mainly to an extensive and fully illustrated paper by W. Beutenmüller on 'The Insect-Galls of the Vicinity of New York City.' It also contains many notes relating to the progress of the museum in adding to or installing its collections. The number contains the index to the volume for 1904.

The Museums Journal of Great Britain has articles on 'A Simple Method of Drilling Glass,' 'The Museum Conference at Warrington,' 'The Hankfield Museum, Halifax,' and many notes from various museums. There is an instalment of the museum's directory of Great Britain which brings the matter down to Manchester.

Bird Lore for November–December contains articles on 'How to Study a Bird,' by Ernest Thompson Seton; 'Some Familiar Florida Birds,' by F. W. Roe; 'Bird Life of a Swiss City,' by Wendell Prime; and 'Young Flamingos,' by Frank M. Chapman. There is the seventh paper on 'The Migration of Warblers,' by W. W. Cooke; 'Bird Lore's Christmas Census' and Notes, Reviews, Editorials and the very full Audubon Department.

SOCIETIES AND ACADEMIES.

THE CONVOCATION WEEK MEETINGS OF SCIENTIFIC SOCIETIES.

THE American Association for the Advancement of Science, the American Society of Naturalists and the following societies will meet at Philadelphia, Pa., during the week beginning December 24, 1904:

The American Association for the Advancement of Science.—The week beginning on December 27, President, Professor W. G. Farlow; permanent secretary, Dr. L. O. Howard, Cosmos Club, Washington, D. C.; general secretary, President Charles S. Howe, Case School, Cleveland, Ohio; secretary of the council, Professor Clarence A. Waldo, Purdue University, Lafayette, Ind.

Local Executive Committee.—President, Provost Charles C. Harrison; vice-president, Professor Edgar F. Smith; secretary, Dr. Philip P. Calvert; treasurer, Dr. Samuel G. Dixon; chairman of the executive committee, Provost Charles C. Harrison; of the committee on reception and entertainment, Mrs. Charles C. Harrison; of the committee on hotels and boarding houses, Professor Amos P. Brown; of the committee on meeting places and

equipment, Professor Edwin G. Conklin; of the committee on press and printing, Mr. George E. Nitzsche; of the committee on transportation, Mr. Walter Wood; of the committee on finance, Mr. S. F. Houston.

Section A, Mathematics and Astronomy.—Vice-president, Professor Alexander Ziwet, University of Michigan; Secretary, Professor L. G. Weld, University of Iowa, Iowa City, Iowa.

Section B, Physics.—Vice-president, Professor Wm. F. Magie, Princeton University; Secretary, Professor Dayton C. Miller, Case School of Applied Science, Cleveland, Ohio.

Section C, Chemistry.—Vice-president, Professor Leonard P. Kinnicutt, Polytechnic Institute, Worcester, Mass.; secretary, Professor Charles L. Parsons, New Hampshire College of Agriculture, Durham, N. H.

Section D, Mechanical Science and Engineering.—Vice-president, Professor David S. Jacobus, Stevens Institute, Hoboken, N. J.; secretary, Professor Wm. T. Magruder, Ohio State University, Columbus, Ohio.

Section E, Geology and Geography.—Vice-president, Professor Eugene A. Smith, University of Alabama; secretary, Dr. Edmund O. Hovey, American Museum of Natural History, New York, N. Y.

Section F, Zoology.—Vice-president, Dr. C. Hart Merriam, U. S. Dept. of Agriculture; secretary, Professor C. Judson Herrick, Denison University, Granville, Ohio.

Section G, Botany.—Vice-president, Professor B. L. Robinson, Harvard University; Secretary, Professor F. E. Lloyd, Teachers College, Columbia University, New York, N. Y.

Section H, Anthropology.—Vice-president, Dr. Walter Hough, U. S. National Museum; secretary, George H. Pepper, American Museum of Natural History.

Section I, Social and Economic Science.—Vice-president, Martin A. Knapp, U. S. Interstate Commerce Commission, Washington; Secretary, Dr. J. F. Crowell, Bureau of Statistics, Washington, D. C.

Section K, Physiology and Experimental Medicine.—Vice-president, Professor H. P. Bowditch, Harvard University.

The American Society of Naturalists.—December 27, 28. President, Professor E. L. Mark, Harvard University; secretary, Dr. Chas. B. Davenport, Station for Experimental Evolution, Cold Spring Harbor, Long Island, N. Y.

The Astronomical and Astrophysical Society of America.—December 28, 29. President, Professor Simon Newcomb; secretary, Professor Geo. C. Comstock, Washburn Observatory, Madison, Wis.

The American Physical Society.—December 30. President, Professor Arthur G. Webster; secretary, Professor Ernest Merritt, Cornell University, Ithaca, N. Y.

The American Chemical Society.—December 28–31. President, Professor Arthur A. Noyes, Massachusetts Institute of Technology; Secretary, Professor William A. Noyes, the Bureau of Standards, Washington, D. C.

The Geological Society of America.—December 29–31. President, Professor J. C. Branner, Stanford University; secretary, Professor Herman L. Fairchild, Rochester, N. Y.

The Botanical Society of America.—December 27–31. President, F. V. Coville; secretary, Dr. D. T. MacDougal, N. Y. Botanical Garden, Bronx Park, New York City.

The Society for Plant Morphology and Physiology.—December 28, 29, 30. President, Dr. G. T. Moore, Department of Agriculture, Washington; secretary, Professor W. F. Ganong, Smith College, Northampton, Mass.

The Botanical Club of the Association.

The Fern Chapter.

Sullivant Moss Chapter.

Wild Flower Preservation Society of America.

The Society for Horticultural Science.—December 27. President, Professor L. H. Bailey, Cornell University; secretary, S. A. Beach, Geneva, N. Y.

The Society for the Promotion of Agricultural Science.—December 26. Secretary, Professor F. M. Webster, University of Illinois, Urbana, Ill.

The Association of Plant and Animal Breeders.

The Association of Economic Entomologists.—President, Professor A. L. Quaintance, Washington, D. C.; secretary, Professor H. E. Summers, Ames, Iowa.

The Entomological Club of the Association.

The American Society of Zoologists (Eastern Branch).—December 27, 28. President, Professor E. A. Andrews, Johns Hopkins University; secretary, Professor Gilman A. Drew, University of Maine.

The American Society of Vertebrate Paleontologists.—December 28–30. President, Professor H. F. Osborn, Columbia University; secretary, Dr. O. P. Hay, American Museum of Natural History, New York City.

The Society of American Bacteriologists.—President, Professor F. G. Novy, University of Michigan; secretary, Professor F. P. Gorham, Brown University, Providence, R. I.

The American Physiological Society.—December

27, 28. President, Professor R. H. Chittenden, Yale University; secretary, Professor Lafayette B. Mendel, New Haven.

The Association of American Anatomists. December 26, 27, 28. President, Professor Charles S. Minot, Harvard Medical School; secretary, Professor G. Carl Huber, 333 East Ann St., Ann Arbor, Mich.

American Folk-Lore Society.

The American Anthropological Association.—December 27–Jan. 2. President, Dr. W J McGee, Washington; secretary, Dr. Geo. Grant MacCurdy, Yale University, New Haven, Conn.

The American Psychological Association.—December 28, 29. President, Professor William James, Harvard University; secretary, Professor Livingston Farrand, Columbia University, New York City.

The American Philosophical Association.—December 28, 29, 30. President, Professor George T. Ladd, Yale University; secretary, Professor H. N. Gardiner, Northampton, Mass.

The Sigma Xi Honorary Scientific Society.—President, Professor S. W. Williston, University of Chicago; secretary, Professor Edwin S. Crawley, University of Pennsylvania, Philadelphia, Pa.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

The 391st regular meeting was held Saturday evening, November 19, 1904. In response to the call for brief notes, B. W. Evermann spoke of the abundance of waterfowl at Lake Maxinkuckee, Ind., on or about November 5, 1904. At that time there were estimated to be on the lake 10,000 coots, 2,500 ducks, including at least 500 canvas backs, 100 to 125 brant and 36 swans.

Dr. E. L. Greene presented a paper entitled 'A Chapter in the Evolution of Generic Nomenclature.' A retrospect over the rise, gradual prevalence and subsequent exclusion from nomenclature of generic names in botany formed by the mere adding of *oides* to the name of an already established genus. For example, the sixteenth century name for the genus *Carex* was *Cyperoides*, for *Festuca* it was *Bromoides*, for *Phaca*, *Astragaloides*; and in the course of the half-century preceding the year 1753 there were taxonomists of excellent standing, like Vaillant, Micheli and Scheuchzer, with whom it seemed to be the rule to construct new generic names in this cheap and easy fashion. There were something like one hundred and fifty or, perhaps, nearer two hundred of these *oides* names prevalent in about the year 1740. At this juncture Linnæus, as if feeling that this kind of name-making was already carried *ad nauseum*, proposed in the 'Philosophia Botanica' that all *oides* names be rejected from plant nomenclature; and in both his 'Genera' and 'Species' he boldly carried the proposal into effect. In this he must have had the full sympathy of almost the whole body of the botanists of that time, for they followed his lead promptly. Only Adanson, whose feeling for Linnæus was bitter, had the hardihood to restore a few of the discredited *oides* names. From 1753 to 1893, not one of the great makers of modern botany adopted an *oides* name.

Unfortunately, by a too ready following of Dr. Otto Kuntze, a few *oides* names, after their long banishment from all botany, have reappeared in American books. In such books *Nicandra* has given place to the earlier *Physaloides*, *Nemopanthes* to *Ilicoides*, *Corydalis* to *Capnoides*, and *Luzula* to *Juncoides*. And so there has begun what seems to be the inauguration of another epoch of this kind of degeneracy in nomenclature. For if *Juncoides* be permitted to stand in place of *Luzula*, other such onomastic deformities will be justified, and what is worse, novices will be found who will take pride in coining new generic names on that very model long ago discredited, and these names will have to be admitted as valid.

In a communication entitled 'A New Seed-bearing Fern,' David White laid before the society specimens and drawings of the sterile fronds and seeds of a new species of *Aneimites*, *A. fertilis*, from the lower Pottsville of southern West Virginia. The genus *Aneimites*, better known under the name *Adiantites*, constitutes the third group of filicoid plants to reveal a seed fructification. It is, therefore, to be referred to the 'Cycadofilices' (Pteridospermeæ), though on account of its typically filicate fronds its fern nature, in the absence of all knowledge respecting its fructification, has hitherto been unquestioned.

Wilfred H. Osgood,
Secretary.

AMERICAN CHEMICAL SOCIETY.

NEW YORK SECTION.

A regular meeting of the section was held at the Chemists' Club, Friday evening, November 11.

Dr. Hugo Schweitzer introduced the following resolution, which was seconded by Mr. T. J. Parker:

> The speedy introduction of the metric system of weights and measures by appropriate laws is most desirable in order to rid our country of a most illogical and cumbersome system, which is one of the greatest obstacles to the development of our export trade, and in order to place our country on a parity with other great manufacturing countries.

After some discussion by Messrs. Peckham, Schweitzer and Parker, the resolution was adopted without dissenting vote.

The regular program was then taken up and the following papers read:

The Determination of Ammonia in Milk. H. C. Sherman and W. N. Berg.

This paper summarized the work thus far accomplished, the preliminary results of which were presented at the Providence meeting in June, 1904.

The Boussingault-Shaffer method (slightly modified) in which the sample is mixed with methyl alcohol, made alkaline with sodium carbonate and distilled under diminished pressure, had been found to be sufficiently delicate for the determination of ammonia in milk where the amount is often less than 0.001 per cent.

With an alkalinity corresponding to 0.5 per cent. sodium carbonate in the mixture of milk and alcohol, a cleavage of ammonia from organic matter appeared to be brought about by this process in the case of stale, but not fresh milk.

If the boiling mixture be saturated with sodium chloride, to reduce the hydrolytic dissociation of the alkali, this cleavage is largely, if not entirely, prevented. The results thus obtained are believed to represent very closely the true amount of preformed ammonia in the samples, while the amount of 'cleavage ammonia' yielded by a sample, when submitted to this process without the addition of salt, appears to be of value as an indication of the condition of the proteid matter.

Methods in which the milk is made alkaline and boiled under atmospheric pressure to expel the ammonia, gave high results even when the dilution of the alkali was very great. Attempts to correct the amount of ammonia given off in a second period of distillation were not satisfactory.

Preliminary experiments indicate that while milk tends to develop both acidity and ammonia on standing, there is no necessary connection between the two, since either may increase rapidly while the other increases slowly, if at all.

Report of Committee on Methods for the Determination of Zinc. G. C. Stone. *Uniformity of Technical Zinc Analyses.*

Early in 1903 the committee sent out three samples of zinc ore: 'A' a pure blende from Joplin containing about 2 per cent. each of iron and lead and 0.3 per cent. of cadmium; 'B' an ore from New Jersey containing considerable amounts of manganese and iron; 'C' a Colorado blende containing about 14 per cent. of lead and 7 per cent. of copper. These were analyzed by forty-two chemists who reported from 56.97 to 59.79 zinc and 2.10 to 3.26 iron in 'A'; from 12.20 to 39.22 zinc and 18.04 to 21.92 iron in 'B'; and from 28.90 to 38.86 zinc and 8.40 to 15.00 iron in 'C.' As the results were so disgraceful the committee have classified the methods in eight groups and discussed them and the results at considerable length. They find that the discrepancies are largely due to the attempt to apply to ores to which they are totally unsuited quick methods that were devised for special cases. For the bad work the committee think the poor instruction given in many of the schools is largely to blame. The only method that gave uniformly good results is that described by Waring (*Jour. Amer. Chem. Soc.*, January, 1904, p. 9); the only reason that they do not recommend its general adoption is that it has only been tried by a very small number of analysts. In conclusion they ask for volunteers to test some of the methods more fully.

In discussing the foregoing paper Professor E. H. Miller pointed out the results obtained at Columbia University on the three samples sent out by the committee, which showed a very good agreement with the standard adopted by the committee, considering that they were done, as requested by the committee, by the method usually employed and without special precautions. Method 8, description of which appeared in the *Journal of the American Chemical Society*, for December, 1903, was discussed at considerable length by Professor Miller. The more important points brought out were: (1) That Waring's statement that the silica should be removed before the precipitation of ferric hydroxide was entirely correct, and the failure to observe this precaution might give rise to an error of 1 per cent. as shown by experiments made by Mr. Falk. (2) That Waring's method for the precipitation of zinc sulphide under pressure is excellent. (3) That the statement that zinc and cadmium can not be separated by hydrogen sulphide is absolutely wrong. The separation can be effected in a hot solution (90° C.) containing one cubic centimeter of concentrated HCl in each fifty cubic centimeters of solution, exactly the conditions given by Fresenius on page 457 of the Nacher's translation. That these conditions must be carefully maintained was shown by a series of experiments made by Mr. Falk to test the separation. (4) That a three per cent. uranium nitrate solution is preferable to uranium acetate as an indicator. (5) That the precipitation of zinc as zinc ammonium phosphate is excellent and was confirmed by the experience of fifteen years in the laboratory. (6) That the separation of zinc and manganese by oxidizing agents requires careful investigation and is not free from error as carried out at present.

The remainder of the evening was devoted to 'A Discussion of Radioactivity,' by Messrs. Wm. Hallock, Hugo Lieber, Jerome Alexander, G. B. Pegram and Charles Baskerville.

F. H. Pough,
Secretary.

DISCUSSION AND CORRESPONDENCE.

CONVOCATION WEEK.

To the Editor of Science: The writer has been interested in the series of letters in Science relating to the affairs of the American Association for the Advancement of Science, and he has an idea that the editor is encouraging these communications as a study in psychology. It is evident that the opinions and suggestions are so very diverse that no plan will be satisfactory to the whole membership. In response to the request for a contribution to the discussion the writer will briefly speak of the general outlook and policy of the association, which is the vital matter that involves all the minor questions of meetings and conduct.

During its earlier life, say from 1840 to 1880, the 'advancement' of science through popular summer meetings was doubtless a useful and successful function of the association; but now the diffusion of scientific education and the great volume of scientific literature have supplanted the association for this work of popularization. For ten years the country at large has paid practically no attention to its meetings. The large cities in which the meetings are held are equally indifferent. The leading citizens will allow their names to be used on local committee lists and will subscribe funds to bring visitors to the town, but not one in ten, probably not one in fifty, go to any session or pay the slightest attention to the meeting, although a few may join the society for a time, and thus give financial aid. The advancement of science through publicity of meetings has become an inconsequential element of the association's work. The reasons for this state of affairs are more or less obvious and are not the fault of the society.

The publication of scientific literature was never an important part of the work of the association, and it has become practically nothing, except as indirectly aided through the columns of Science.

While the sections of the society are still active it must be admitted that in the case of several sections this is only through the

cooperation or 'affiliation' of other special and independent societies.

The social function of the association is not alone sufficient to keep it effective. Comparison with the British Association is valueless, since the geographical and social conditions are entirely different.

The old days and old ways of the American Association are gone. It is natural that the older or more conservative members should feel regret over the changes and the evident trend and the loss of the pleasant summer meetings. But it is wiser to recognize the facts and adapt ourselves to the change than to shut our eyes to the handwriting on the wall. Scientific societies can not escape from the rapid and radical social evolution of the time, no matter whether we regard the changes as good or bad.

The association is still moving in a general way along old lines by virtue of its acquired momentum, but new outside forces are pushing it from its course. It is now upheld largely by the tacit cooperation of special societies and by the reverence and affection of men of science for the old, national, parent society. But knowledge is now so vast and diverse and the intellectual and economic forces so strong that specialization is inevitable, and no one society can expect to include the whole field.

The active, successful administration of the society during the last few years has greatly increased its membership, chiefly by securing the adherence of scientific men. This indicates a fundamental fact, that henceforth the association will be and ought to be conducted by and in the interests of men professionally scientific, with less deference to 'advancement,' of science by popular features. The 'American Science Association' would be a better name.

The above seem to the writer as basal facts which must be recognized in any wise planning for the future. The association may remain a great scientific and educational power if rightly conserved and directed. But, no matter what fine schemes individuals may devise, no one can clearly see the path more than one step in advance. The safest way is to trust the matter to a wise council, which should move slowly and feel the way and meet demands for change as they arise.

The association may properly become the central organization or national representative of the many special societies. If the association should be withdrawn from its present relationship to the special societies they would find it desirable to create a central body through which they might act and speak collectively. Some general organization is essential. This idea of the function of the association has already been recognized by giving the 'affiliated' societies the privilege of proportionate representation on the association council. In any scheme of consolidation the matters of association membership and finance are the most difficult to adjust, but they can be arranged satisfactorily when the necessity arrives. As sections are supplanted by special societies their ordinary scientific meetings might be suspended. However, the organization of the sections should be retained, at least for a time, for administrative purposes or other needs which may develop. The chairmanships and secretaryships of the sections would lose nothing of their honorary character because meetings for reading of papers are not held. The presidency and other general offices of the society would be even more dignified as being more broadly and powerfully representative of American science.

For some years the association and the special societies have been drawing together. The wise course is to let the evolution proceed naturally, a step at a time, as the road opens before us, and not to allow any strong personality nor any group of men to force a hasty decision on important matters. We should go slowly, but keep moving.

Here is one suggestion for immediate use. The council should be kept thoroughly representative not only of the association membership but of the affiliated societies. *A strong, wise, harmonious and not too radical council is the most important present consideration.* To this end the sections should be emphatically advised to select for officers and council representatives their most experienced and

wisest men, disregarding at this juncture mere scientific claims. The council should select for presidents at this time not representatives of science as merely to do honor but men of large experience and sympathy with the association affairs. It is of less consequence what the public thinks about the association than what we shall do for ourselves.

Realizing the gravity of the condition at this critical time the council will make wise decisions only after full discussions in a generous spirit; and the membership should in patience trust the collective wisdom of the council.

HERMAN L. FAIRCHILD.

ROCHESTER, N. Y.,
December, 1904.

'THE PROBLEMS OF EXPERIMENTAL PSYCHOLOGY.'

TO THE EDITOR OF SCIENCE: On p. 788 of your issue of December 9 (second column, line 8), I am made to speak of 'classification *a posteriori*.' What I wrote, what the sense requires, what I saw in proof, and what I left in proof, was 'classification *a potiori*.' On p. 794 (bottom of first and top of second columns), I am made to say: 'we analyze and trace to their conditions total consciousness.' What I wrote, what grammar requires, what I saw in proof, and what I left in proof, was 'consciousnesses.' A little knowledge, even in a proof-reader, is a dangerous thing.

E. B. TITCHENER.

CORNELL UNIVERSITY,
December 10, 1904.

[The errors probably would not have occurred if Professor Titchener had returned his proof to the editor in accordance with the instructions accompanying it. It was sent directly to the printers.—ED.]

SPECIAL ARTICLES.

A SUGGESTION LOOKING TOWARDS ULTRA-MICROSCOPY.

THE visibility of an object both to ordinary vision and when helped by telescope or microscope depends upon a favorable combination of several physical conditions. (1) The object must send us ethereal waves whose lengths lie between the limits of 0.38 and 0.76 microns or the violet and red ends of the spectrum respectively. (2) The difference between the intensity or color of these waves and those coming from the adjacent background must be appreciable to our nervous system. (3) The focus on the retina must be sharp. (4) The duration of the image on the retina, or, as the photographer would say, the length of the exposure, must be long enough to enable the brain to appreciate the details of the image.

By means of photography we are able to make long exposures and the fourth condition can be satisfied to such extent that fleeting pictures are caught by instantaneous exposures, while the faintest nebulæ and stars are caught by exposures that last many hours. Becquerel's first photograph by the rays that are called after his name was by an accidental exposure of many days.

By means of the schleier method, originally due to Foucault, we can overcome the difficulties of the second condition and photograph moving air waves when properly illuminated, and this method can be applied to microscopic objects and liquid substances as well as to the larger motions of the air that have been photographed by Mach, DuBois and others.

The ultimate limit of visibility is also defined by the second condition or the wave-length and intensity of the illuminating light that can affect the retina, or the sensitized photographic plate. An object that is visible by monochromatic violet light may not be visible by monochromatic red light or *vice versa,* just as a body that can vibrate to a given high pitch is often too small to send out a low note. An ear that is too dull to hear the low notes may hear a high pitch. Our retina is so constructed as to be insensible to ultra-violet light, but we can by fluorescence make short waves become visible, *i. e.,* an object illuminated by ultra-violet light whose wave-lengths may be anywhere from zero to 0.38 microns may be too small to be directly affected by long waves, but will, by fluorescence convert the short waves into longer ones whose lengths may be any given multiple of the ultra-violet wave, and will,

therefore, be visible to us if the length of the multiple lies between 0.38 and 0.76.

If, therefore, for botanical, physiological and bacteriological work we stain our preparations with fluorescent substances and illuminate with ultra-violet light, we shall bring into prominence smaller particles or structures than can possibly be seen with the ordinary white or colored light. Or, if we prefer, instead of the retina we may expose a sensitized plate, especially one that is sensitive to the particular fluorescent waves excited by the special ultra-violet light that we employ.

But the success of this modification of the ordinary methods depends also upon having our microscope lenses so ground as to correct for the particular waves that we are employing. In fact, one may conceive that an ultra-violet wave of, for example, 0.09 microns may by fluorescence excite a wave of 0.18, or 0.27, or 0.36 microns, and will, therefore, still be invisible to the eye, while perfectly competent to do photographic work. It will, therefore, be a great labor to grind the lenses properly, since their perfection can only be tested by experiments with invisible fluorescent rays; but when perfected these lenses and the photographs give us a power of what we may at present call 'ultra-microscope research.' The development of such work is limited only by the chemical and physical properties of atoms and molecules, and is not in any way affected by the limitations of the human eye.

The first steps toward realizing this advance in microscopy will naturally be made with the ordinary microscope, and the ordinary soluble fluorescent substances, among which we recall eosine, thallene, quinine, æsculine, chlorophyll, magdala red. If now with the fluorescent staining and ultra-violet illumination we combine the principles of the schleier method it would seem that there will in the future be no limit to the powers of research, except that which is set by the diffraction phenomena. The ultimate limit of actual photographic visibility will be of the dimension of one or two of the very shortest wave-lengths of ether, or of the same dimensions as the larger molecules themselves.

It is possible that Professor Ernst Abbe, of Jena, and his colleagues have been working along some line of thought similar to the preceding, as I notice that in the list of scientific instruments in the German educational exhibit at the St. Louis Exposition, page 213, mention is made of the ultra-microscopic work, but from what little is stated it would seem possible that this refers especially to the ordinary microscope combined with the schleier method; I have not as yet learned of any details and may be entirely wrong. The flood of ultra-violet light given out by the soft-iron electrodes of Dr. Piffard's tube and the magnificent fluorescent effects displayed by Mr. Geo. F. Kunz in his lectures suggested to me the preceding combination of fluorescence, the schleier method, and the microphotograph, and I have been encouraged to publish the idea.

To-day an interview with Dr. Sigfried Czapsky, a colleague of Professor Abbe's, has brought to my attention the fact that great improvements in ultra-microscope work are in progress at Jena, but not yet sufficiently developed to justify publication. I have, therefore, taken the liberty of sending you this communication in hopes that there will be some suggestion in it worthy the attention of these eminent opticians.

Cleveland Abbe.

Washington,
November 6, 1904.

EXTINCT PEDICULATE AND OTHER FISHES.

While engaged on chapters relative to the Pediculates for a work on fishes, I was extremely pleased to receive just what I wanted—further information respecting the former history of the order. I had received from Dr. C. R. Eastman an interesting 'Bulletin of the Museum of Comparative Zoology' entitled 'Descriptions of Bolca Fishes.' In that were figures and description of a form referred to '*Lophiidæ*' and named *Histionotophorus bassani* (Zigno). A close examination of the reproductions of photographic figures, however, convinced me that the fish was not one of the lophiids but a typical antennariid. The mode of fossilization showed that it was a com-

pressed fish and the structure and relations of the fins to each other and the interspinals, as well as other skeletal details, conclusively demonstrated that the animal was closely related to the existing *Antennarius* and *Pterophryne*. Indeed, with skeletons of those forms before me, after elimination of faults of restoration, I could find no generic differences between the fossil and the recent *Pterophryne*. The caudal of the extinct species, it is true, was longer and that difference may be correlated with others observable by close scrutiny, but the figures published do not give details with sufficient clearness to appreciate them.

Still more apt than Dr. Eastman supposed, then, are his remarks: "Attention should be called * * * to the remarkable fact of a type of fish-life appearing suddenly in the Eocene, already highly modified, without any known predecessors nor any that can be plausibly conjectured, but which persists after its first introduction essentially unchanged until modern times."

The recognition of the relationship of '*Histionotophorus*' accentuates the deduction. We now have two (the only known) eocene Pediculates so nearly related to greatly differentiated recent forms that their generic differences, if any, remain to be discovered—the *Lophius brachysomus* and *Histionotophorus* (perhaps *Pterophryne*) *bassani*. Yet the Pediculates are exceptionally aberrant and specialized fishes. The significance of the facts may be appreciated when it is recalled that nearly all the contemporary mammals belonged to extinct families or, conversely, that almost all the recent families have been evolved since—so far as we know.

The history of the form is noteworthy. De Zigno (1887) recognized its similarity to the genus *Antennarius* (Questa forma * * * presenta qualche somiglianza col genere *Antennarius* di *Commerson*) but distinguished it by its form, horizontal mouth and elongated fin-rays, especially those of the caudal. Smith Woodward, without reference to the views of De Zigno, noticed it, among the 'Scorpænidæ,' as one of the 'extinct genera and species, which are not represented in the collection, [and] are supposed to be related to *Scorpæna*.' Eastman's view has just been given. It only remains to add that no such differences as are urged by De Zigno exist, except the long caudal rays. The horizontal mouth is the result of distortion through pressure. The figures marked on Eastman's plate (I.) '1.' and '1a' show the subvertical mouth.

One more of several points raised by Dr. Eastman's important memoir may be alluded to.

Another Monte Bolca fish has been described and photographed as *Symphodus szajnochæ* (= *Crenilabrus szajnochæ* Zigno). The non-labrine appearance and osteological characters led me to read the description. It appears, according to Dr. Eastman, that 'there are at least eight branchiostegal rays' and that 'the scales are thin, ctenoidal and very strongly pectinated.' It is, therefore, evident that the species is not even related to the labrids. The characters specified are rather those of berycids, but it belongs to no known genus and the family even is uncertain. The combination of form, rounded caudal, single dorsal with eleven stout spines, and anal opposite soft dorsal, with ctenoid scales and increased branchiostegal rays, separate it from any other fish, so far as known, and it may be differentiated as a distinct generic type and named *Bradyurus*. It is to be hoped that Dr. Eastman may re-examine the fish and give the results of his review.

THEO. GILL.

THE RE-DISCOVERY OF DINOMYS.

THE great rat-like rodent *Dinomys* was discovered in 1873 in the Peruvian Andes, and since that time the specimen, which is preserved in the Berlin Museum, has remained unique. In the spring of this year Dr. Goeldi, of the Museum of Para, announced the re-discovery of this rare animal in the lowlands of Brazil. The following notes as to its appearance and habits are abridged from Dr. Goeldi's account of this animal which appeared in the *Proceedings of the Zoological Society* for May and June.

The general build of *Dinomys* is thick-set and inclined to corpulency. Due to the fact of setting

the whole plantigrade sole on the ground, the hind feet especially, the *Dinomys* has a waddling gait, and reminds one of an immense rat well advanced in development towards a bear.

The predominant feature of the character of *Dinomys* is a combination of leisurely movements and supreme good nature. It knows absolutely nothing of haste. Spending the greater part of the day sleeping in a corner—the mother often lying upon the young one, or standing over it, as if to protect and to keep it warm—opening its half-closed eyes only when it hears the approaching steps of the keeper, it forms the resolution to move with slow gait, expecting some food, evidently governing its movements as much by hearing and smell as by sight. It is not easily irritated, and permits one to stroke and to scratch its head and back, and only occasionally manifests its displeasure by a low guttural growl. I have never yet observed a manifest intention to bite. When let out of the cage it makes no attempt to escape, and limits its excursions to an exploration of the immediate neighborhood in search of something to eat. It occasionally scratches itself rapidly with its long claws, which is the only occasion on which it manifests a capacity for rapid movements when required. One thing not yet definitely verified by us is its proclivity for digging, the development of the claws at least leading to the supposition that the animal is well fitted for that purpose. The amiable relations always existing between mother and son prepossesses one most favorably as to the natural disposition of the animals.

As matters now stand, it would be justifiable to suppose that the true home of *Dinomys* is not properly in the Peruvian Andes, and that the first specimen found there was merely a stray individual and that its actual habitat may rather be located in the almost unexplored regions of the eastern slopes and tablelands of the Bolivian and Peruvian foot-hills bordering on Brazil, including geographically the head-waters of the rivers Acre, Purûs and Juruá.

F. A. LUCAS.

CURRENT NOTES ON METEOROLOGY.

MONTHLY WEATHER REVIEW.

THE two latest issues of the *Monthly Weather Review* (July and August, 1904, dated September 19 and October 21 respectively) contain the following papers of general interest: 'The Movements of the High Clouds in the West Indies,' by J. T. Quinn; 'Attempts at Methodical Forecasting of the Weather,' by L. Besson (translated from the French); 'Air Radiation,' by C. C. Hutchins and J. C. Pearson, of Bowdoin College; and notes on 'Meteorology at Montpellier, France,' 'Early American Weather Records,' 'Weather and Crops in Arizona,' 'The Climate of Manila,' 'Secular Changes in Climate,' 'The Capacity of the Air for Aqueous Vapor,' 'Temperature of the Upper Atmosphere,' 'Precipitation in Wisconsin,' 'Meteorology in Chile' and 'Cannonading against Hail.' Further, 'The Annual and Geographical Distribution of Cyclones of High Velocity in the United States, 1893–1902,' by Stanislav Hanzlik; 'Dust in the Atmosphere during 1902–03,' by Andrew Noble; 'The Origin of the Cuba Cyclones of June 13–14, 1904,' by Maxwell Hall; and the following notes: 'The Primary and Secondary Rainbows,' 'Formation and Movement of Hurricanes,' 'A Legal Decision as to Damage by Lightning and Wind,' 'Are the Movements of Thunderstorms deflected by the Tide?' and 'The Diurnal Variation of the Barometer at Milwaukee.'

CHANGES IN BLOOD AT HIGH ALTITUDES.

DR. K. BURKER, of the Physiological Institute of Tübingen, has been making an experimental study of the physiological effects of high altitudes at the Schatzalp Sanatorium, 6,119 feet above sea-level. In the case of rabbits brought from a lower level, and kept for different lengths of time at 6,000 feet, an increase of 25 per cent. in the amount of iron in the blood was noted. The liver showed first an increase of iron; then, after a longer time at the greater altitude, a decrease, and in the case of rabbits kept still longer, there appeared to be less iron than in the livers of rabbits at Tübingen. In a similar line are the studies of the blood of human beings made by Dr. Gaule during two balloon trips. The effect of the balloon trips was to increase the number of red corpuscles of each of the persons examined. Similar results have previously been obtained by Viault, Müntz and others.

AN INSTRUMENT FOR DETERMINING WIND AT SEA.

IN the *Quarterly Journal of the Royal*

Meteorological Society, October, 1904, Mr. A. L. Rotch, of Blue Hill Observatory, describes an instrument for determining the true direction and velocity of the wind at sea, devised by himself and constructed by Casella, of London. With this instrument the angles of the apparent and true wind relative to the ship are measured directly, and by utilizing the ship's course and speed as a base, absolute directions and velocities of both winds are immediately ascertained.

GENERAL CIRCULATION OF THE ATMOSPHERE.

The report by Hildebrandsson, to the International Meteorological Committee, on the international cloud observations, the principal conclusions in which were some months ago referred to in these 'Notes,' is published in English in the *Quarterly Journal of the Royal Meteorological Society*, Vol. XXX., October, 1904. This study has attracted much attention because of the new views advanced in it concerning the general circulation of the atmosphere, and it is well to have it accessible to a larger number of readers than was the case with the original publication in French.

KITE-FLYING AT SEA.

During the past summer, the Prince of Monaco has been investigating the meteorology of the free air in the northeast trade wind latitudes. Kites have been flown from the yacht *Princess Alice*, and an altitude of nearly 17,000 feet was attained on one occasion. In this kite work, Dr. Hergesell was actively interested and he accompanied the expedition, but Americans will recall that the first suggestion concerning the use of kites for exploring the atmosphere over the oceans was made by Mr. A. L. Rotch, of Blue Hill Observatory.

METEOROLOGICAL INSTITUTE OF SAXONY.

The 'Jahrbuch' of the Royal Meteorological Institute of Saxony for 1900, compiled by Dr. Paul Schreiber, contains an elaborate critical discussion of the pressure observations made in Saxony between 1866 and 1900, as well as the meteorological summary for the year 1900, with special discussions of evaporation measurements, thunderstorms and depth of snowfall.

R. DeC. Ward.

BOTANICAL NOTES.

STUDIES IN PLANT FECUNDATION.

A very useful compilation representing the present state of our knowledge of the process of fecundation in plants has recently appeared from the hand of Professor D. H. Mottier in one of the publications (No. 15) of the Carnegie Institution of Washington, under the title of 'Fecundation in Plants.' It is a thick octavo pamphlet of nearly two hundred pages, with seventy-five text illustrations. The author's purpose is well stated in the preface to be 'to present the subject of fecundation in the vegetable kingdom by the discussion of concrete cases, selecting from the great groups of plants certain typical representatives in which the sexual process seems to have been most thoroughly investigated.' In carrying out this purpose he devotes an introductory chapter of sixty pages to the discussion of typical problems of nuclear division and cell formation, especially in spore mother-cells, closing the chapter with an interesting ten-page discussion of the significance of the sexual process. In the latter the author is very emphatic in his disbelief in a chemical theory of fecundation. "Although the development of a rudimentary embryo induced by artificial means may proceed in the same manner as the product of normal fecundation, yet the artificial stimulus can not be looked upon as being equivalent to the sexual process. In the case of the former, we are dealing with a stimulus which merely starts growth, but a mature individual is never developed. The sting of an insect or some similar stimulus may call forth a growth in a leaf of an oak which results in a gall, a local and limited growth, but never in an oak tree, and we can not for one moment think of comparing such a stimulus to a sexual process." And again, 'The author does not agree with those who regard the sexual process merely as a restoration to the egg of the power of growth and division.'

The second chapter includes the discussion of typical cases of fecundation in which motile isogametes are concerned, the examples selected being *Ulothrix*, *Hydrodictyon* and *Ectocarpus*. Here he shows 'that fecundation

consists in the fusion of the sexual nuclei together with the cytoplasm of the gametes.' In Chapter III. fecundation by non-motile isogametes is considered, as illustrated by *Sporodinia, Closterium, Cosmarium,* the Diatoms (*Rhopalodia, Cocconeis*) and *Basidiobolus.* In Chapter IV. the heterogamic fecundation of *Sphæroplea,* the Fucaceæ, *Volvox, Œdogonium, Coleochæte, Vaucheria, Albugo, Achlya* and *Saprolegnia* are discussed. Then follow chapters on 'the type of the *Ascomycetes* and Rhodophyceæ (V.), 'the Archegoniates' (VI.), including Pteridophyta and Gymnosperms, and 'Angiosperms' (VII.). In this last chapter the author says, 'the view held here is that pollen grains and embryo-sacs are respectively micro- and macrospores.' Here the author has permitted some confusion to creep into his usually lucid text, for a little later he uses the expression 'the embryo-sac or female gametophyte' (pp. 169 and 173). Certainly an embryo-sac can not be both macrospore and female gametophyte. A voluminous bibliography, including 187 titles, closes the paper, which can not but prove to be very useful to botanical teachers and students.

TECHNICAL MYCOLOGY.

SEVEN years ago Doctor Franz Lafar, private docent in the technical high school at Hohenheim, issued the first part of a most useful work under the title 'Technische Mykologie.' About a year ago the second part appeared, and with it the announcement that the present edition was to be discontinued, and that a second, much enlarged edition was to be undertaken immediately. Of this new edition the first *Lieferung* of 160 pages has just appeared. It includes an introduction of 28 pages—mostly historical—by Dr. Lafar, followed by 121 pages by Dr. Migula devoted to the bacteria. Near the close of the *Lieferung* we have the beginning of Dr. Lindau's treatment of the true-fungi (*Eumycetes*). If one may judge from these pages, this edition is to be a notable addition to mycological literature. From the prospectus it is learned there are to be five volumes, the first of which is to be devoted to the general morphology and physiology of the fungi. The succeeding volumes are to be more or less technical, dealing with the relation of the fungi to various industries, the soil, water-supply, etc.

PARTHENOGENESIS IN PLANTS.

IN a recent number of the *Berichte der deutschen Botanische Gesellschaft* (Vol. XXII.), Dr. J. B. Overton describes the cytology of parthenogenesis in *Thalictrum purpurascens,* a common American weed. The author has prepared a summary of this paper, as follows:

In a previous experimental and morphological study the author discovered that this plant sets seed freely in the absence of pollination and that the embryos could develop normally from fertilized egg-cells and also parthenogenetically from unfertilized egg-cells. In the present investigation the author studied the cytological phases of the subject. He was able to determine that the number of chromosomes was reduced by one half in the pollen-mother-cells by means of the heterotypical mitosis. Similar conditions were also found in the typical embryo-sac mother-cell, which gives rise to true tetrads. He also found in exceptional cases embryo-sac mother-cells in which no reduction takes place. The division, instead of being heterotypical, appears to resemble both vegetative and heterotypical mitoses. This division, therefore, represents a transitional stage between the ordinary somatic and the heterotypical divisions. Instead of twelve chromosomes, the reduced number, the spindle shows twenty-four chromosomes. In those embryos which developed from normally fertilized eggs as well as those which developed parthenogenetically, he found twenty-four chromosomes. Twenty-four chromosomes were always found in purely vegetable cells. The author concludes that only those eggs with the somatic number of chromosomes are able to develop parthenogenetically, while those which contain the reduced number of chromosomes must be fertilized. The works of Juel and Murbeck show that *Antennaria alpina,* several species of *Alchemilla* and *Taraxacum officinale,* have acquired the habit of complete parthenogenesis. *Thalictrum purpurascens* has only partly attained the power of propagating itself parthenogenetically. The author believes it possible that the failure of pollination, due to a separation of staminate and pistillate plants, has acted as a stimulus to final parthenogenetic development.

THE WESTERN SAND CHERRY.

Most eastern readers are well acquainted with the low, much-branched shrub known as the sand cherry (*Prunus pumila*), and bearing small fruits which have a thin flesh. Probably few of them are familiar with the western sand cherry (*Prunus besseyi*) which grows on the prairies and great plains from Manitoba and Minnesota to Nebraska and Kansas. The latter has a much larger, short-stalked cherry, which has a colored, juicy flesh. The plants of the two species are much alike, but there are constant differences in habit and fruit sufficient to warrant their separation. For practical purposes the two are very distinct, the western species, even in the wild state, being valuable for culinary purposes.

In a recent bulletin (No. 87) of the North Dakota Experiment Station, Professor Hanson makes a discussion of this fruit, giving the results of his experiments extending over fourteen or fifteen years. These may be summarized as follows:

1. It is exceedingly variable in the size and quality of its fruit, but all forms are acceptable for culinary use.

2. From them 100 varieties have been selected, and are now under propagation. Some of these bear fruit from three fourths to seven eighths inches in diameter and of a quality acceptable for eating without cooking.

3. It hybridizes readily with at least three other species.

4. Seedlings bear fruit in the third year.

5. "After fruiting many thousands of seedlings it appears reasonable to believe that in this species we have a bush cherry that can be raised to advantage upon the most exposed prairies."

As to the present value of this species, Professor Hansen says:

1. "It is a native northwestern prairie fruit worthy of being tamed and transferred to the small fruit garden."

2. "It is yet in the early stages of development; too much must not be expected at first."

3. "Even unselected seedlings are not to be despised in the drier regions of the northwest, where the small fruits of the eastern states are usually a failure."

4. "At least one of its hybrids (the 'Compass,' = Sand Cherry × Miner Plum) is worthy of a place in the home garden, and may be considered the forerunner of a new race of fruits."

5. "Propagators will find the sand cherry worthy of attention as a stock in winter root-grafting of the native plum."

6. "For orchard houses and amateur plantations it can be used to advantage, as a dwarf stock for plums, peaches, apricots and some other stone fruits."

7. "It is worthy of a place on the list of desirable low ornamental shrubs for the foreground in clumps of larger-growing species."

The professor's experience with the sand cherry has been so favorable that he ventures the following predictions:

1. "It will be found of great value in the commercial propagation of some of the stone fruits."

2. "From it will be developed by selection a race of bush fruits with fruits equal to California cherries in size, and of quality acceptable for table use."

3. "From it will be developed a race of hybrid fruits of a new type by hybridizing with choicer fruits: these new creations will be hardy and fruitful on the most exposed prairies."

Charles E. Bessey.
The University of Nebraska.

*COLLEGE-ENTRANCE OPTION IN ZOOLOGY.**

The following report will be presented to the American Society of Zoologists at the meeting at Philadelphia in December. It is published here in order to call forth criticisms and suggestions from schools and colleges which have not been reached through correspondence. Communications relating to the report may be handed to any member of the undersigned committee, or addressed to the chairman at Teachers College, Columbia University.

Believing that zoology should have a place in general liberal education and recognizing that for the great majority of citizens formal

* Report of a committee appointed by the American Society of Zoologists.

education must end with the secondary school, it is the opinion of this committee (1) that this science should be taught in high schools for the benefit of pupils who will have no other opportunity of acquiring general knowledge of animals; and (2) that zoology thus taught from the point of view of general secondary education should have recognition as a college-entrance option, in order that pupils who can not decide to go to college before the close of high-school work may not be held deficient in credits because zoology was elected. But, although thus recognizing zoology from the standpoint of secondary education, this committee wishes to emphasize the opinion that zoology is not one of the most desirable subjects as preparation for college; and that the physical sciences should first of all be recommended to pupils who expect to go to college.

In reaching these conclusions the committee has not failed to consider the value of general acquaintance with common animals and with the essential principles of the elementary physiology and hygiene of the human body, but these are commonly taught in the years below the second of high school, and work in these lower years does not in the case of other subjects closely concern the question of college-entrance options.

These preliminary statements will make it clear that the following suggestions for a scheme of college-entrance credits in zoology are intended by this committee simply to provide for crediting the zoology which should be elective in every good general high school, but not to advocate the subject as one which from the college standpoint is desirable in preparation for college. In other words, this committee is simply recommending that zoology studied as part of a liberal secondary education intended primarily to prepare for life should be recognized as preparing for entrance to college (the minor question of preparation for entrance to college courses in zoology being here laid aside).

General Statement of Options.—(*a*) One-point option. To count as one unit or point in thirteen to fifteen required for entrance to college. This should consist of one year of at least five hours per week devoted to study of zoology, as indicated in outline of course given below.

(*b*) Half-point option (one point in biology). To count as one half unit in thirteen to fifteen required, only when a half unit of botany taken in the same continuous course is offered to complete a full unit in biological science. A half year of zoology independent of botany should not be accepted.

A two-point option is not recommended, because more than one full year in zoology is extreme specialization which no secondary school can properly undertake.

Outline for the One-Point Option in Zoology.—The following outline includes the principles of zoology which are indispensable to a general survey of the science. It is not intended to indicate order of study of the topics—this must be left to the teacher and the text-book. With little modification the courses presented in general books such as Needham's, Kingsley's, Kellogg's and Colton's (revised) cover the ground outlined below.

1. The general natural history—including general external structure in relation to adaptations, life histories, geographical range, relations to other plants and animals, and economic relations—of at least one animal of each prominent order of vertebrates and one of each prominent class of invertebrates so far as representatives of these groups are obtainable in the locality where the course is given. In the case of arthropods, pupils should become familiar with common crustaceans, spiders, myriapods, and insects representing at least five orders. Actual examination of common animals with reference to the above points should be supplemented by reading giving natural-history information equivalent to that in recent books by Davenport, Jordan & Heath, or Kingsley.*

2. The classification of animals into phyla and leading classes (except the modern subdivisions of the worms) and the great characteristics of these groups. In the case of in-

* A large part of this natural-history information will be gained from the nature-study of the elementary school and from the course often given in the first year of high school.

sects and vertebrates the characteristics of the prominent orders. The teaching of classification should be by practical work so as to train the pupil to recognize animals and to point out the chief taxonomic characteristics. The meaning of species, genera and larger groups should be developed by constructive practical work with representatives of insect or vertebrate orders.

3. The general plan of internal structure, not the anatomical minutiæ, of one vertebrate (preferably frog or fish) in general comparison with human body; an arthropod (preferably a decapod); an annelid (earthworm or Nereis); a cœlenterate (hydroid, hydra or sea-anemone); a protozoon (a ciliate, and amœba when possible). In place of any of above types not locally available there may be substituted a second vertebrate, an insect or an echinoderm. Tissues should be examined first with the unaided eye, in such a structure as a frog's leg, and then with a microscope demonstrate the relations of cells and intercellular substance in epithelium and cartilage; and, if possible, in other tissues. The functions of the chief tissues and their positions in the body of a vertebrate should be pointed out.

4. (*a*) The general physiology of above types, involving the essentials of digestion, absorption, circulation (respiration), cell-metabolism, secretion, excretion and nervous functions. This should apply comparatively the essentials of elementary work in human physiology (see chapters 8, 9 and 10 in Martin's 'Human Body, Briefer Course'). Demonstrations and experiments, such as are suggested in high-school text-books on human physiology, should be introduced, or recalled if not previously well presented in elementary physiology, in connection with the discussion of the chief functions. As far as practicable structure and function should be studied together.

(*b*) Comparison of the general life-processes in animals and plants (in connection with botany if zoology is first studied).

5. The very general features of asexual reproduction of a protozoon (preferably *Paramœcium*); alternation of generations in hydroids; reproduction and regeneration of *Hydra;* the very general external features of embryological development in a fish or frog; and the general cellular nature (not centrosomes and the like) of germ-cells, fertilization and cell division in developing eggs should, as far as possible, be demonstrated and briefly described. Also, the most interesting features of development should be pointed out in the case of other animals studied. (The limited microscopic work suggested above might all be carried out with only one microscope for demonstrations.)

6. The prominent evidences of relationship, suggesting evolution, within such groups as the decapods, the insects and the vertebrates, should be demonstrated. A few facts indicating the struggle for existence, adaptation to environment, variations of individuals and man's selective influence should be pointed out; but the factors of evolution and the discussion of its theories should not be attempted.

7. Some leading facts regarding the epoch-making discoveries of biological history and the careers of such eminent naturalists as Darwin, Huxley, Pasteur and Agassiz should be presented.

The above outline of a course in general zoology should be developed on the basis of a course of laboratory study guided by definite directions. This should be supplemented by the careful reading of at least one modern elementary text-book in general zoology. At least two thirds of the time should be devoted to the practical studies of the laboratory. If good nature-studies have not preceded the course in high-school zoology, pupils should be encouraged to do supplementary work in the line of natural history. A note-book with carefully labeled outline drawings of the chief structures studied, and with notes on demonstrations and in explanation of drawings, with dates and an index, must be submitted, properly certified by the teacher, at the time of the examination. It will be graded as one third of the examination.

The question whether a course as outlined above should admit students to the second college course in zoology is one which must be answered by each college for itself. It is

quite unimportant so far as accepting an entrance option in zoology is concerned, for the very few pupils who study the science in high school and later in college have special interests which make adjustment of their college work easy.

Outline for the Half-Point Option.—(1) The general natural history specified above. (2) The classification of animals specified above. (3) The general internal structure of one vertebrate and a decapod or annelid. (4) The physiology of these two animals along the lines suggested above, with special application to the functions of the human body, and comparison with the general functions of plants. (5) The general external embryology of frog as suggested above.

Committee: C. M. CLAPP,
E. G. CONKLIN,
C. W. HARGITT,
J. S. KINGSLEY,
M. A. BIGELOW, *Chairman.*

THE JOHN BELL SCOTT MEMORIAL OF WESLEYAN UNIVERSITY.

THE John Bell Scott Memorial, the physical laboratory of Wesleyan University, was dedicated on December 7. The building was presented on behalf of the building committee by Dr. H. C. M. Ingraham and a response was made by President Raymond. The principal address was made by Dr. Edward B. Rosa, formerly professor of physics at Wesleyan University and now physicist of the National Bureau of Standards. The address, which will be published in SCIENCE, was on 'The National Bureau of Standards in Relation to Scientific and Technical Laboratories.'

The John Bell Scott Memorial is a gift to Wesleyan University from the late Charles Scott, of Philadelphia, and his son Charles Scott, who died from disease contracted while serving as chaplain of the U. S. Cruiser *St. Paul,* during the Spanish-American War. The building is a handsome structure of Harvard brick and Indiana limestone, the architect being Mr. Charles A. Rich, of New York City, well known in college circles for his exceptional success as the architect of the splendid new group of college buildings at Dartmouth. The main part of the building is 102 x 51 feet on the ground plan, and this part consists of basement, three stories and attic. In addition there is an extension of 50 x 30 feet in the rear which has basement and two stories. The lecture room is situated on the second floor, running out into the extension, is 44 x 40 x 17 feet in size and seats nearly 200 persons. A smaller lecture room on the third floor has a seating capacity of about forty. There are in the building twenty-two rooms which are more distinctively for laboratory purposes, in addition to the lecture rooms, offices, photographic dark rooms, store rooms, apparatus rooms, etc. For experiments which require great vertical space, a tower has been provided about 4 x 6 feet in cross section and with a height of about 54 feet in the clear. The building is abundantly supplied with water and gas connections throughout and is exceptionally well equipped with a system of wiring for distributing to all points alternating and direct current from the city mains and also direct current from the storage battery room in the basement.

THE GERMAN METEOROLOGICAL AND MAGNETIC OBSERVATORY IN THE SAMOAN ISLANDS.

DR. FRANZ LINCKE, of Göttingen, Germany, has been appointed to take charge of the German Meteorological and Magnetic Observatory at Apia, Samoan Islands, thus relieving Dr. Tetens, who returns to Germany in order to reduce the records obtained during the past two years. This observatory is equipped with the most modern instruments for observations in meteorology, terrestrial magnetism, atmospheric electricity and seismology. In view of the important location of this station and the opportunity presented to supplement the data obtained at the Coast and Geodetic Survey Magnetic Observatory in the Hawaiian Islands, situated on the opposite side of the magnetic equator from that of the Samoan station, the German government has decided to further continue its observatory. It was the original intention to conduct the work only during the time of the German and British Antarctic expeditions.

En route to Samoa, Dr. Lincke stopped at Washington, November 17–21, and compared a set of portable magnetic instruments with the Coast and Geodetic Survey standards at Cheltenham, Maryland, he having previously compared the same set with the Potsdam magnetic observatory standards. Upon his arrival at Apia, he will compare this set with the observatory instruments used there and thus secure the necessary data for correlating the various observatory standards. Arrangements are thus being perfected for effective cooperation between the magnetic observatories of the German government and those of this country.

A MONUMENT TO J. W. POWELL.

During the recent excursion of the Eighth International Geographic Congress to the Grand Canyon of the Colorado in Arizona, a meeting was held in memory of Major J. W. Powell, in which his exploration of the canyon, his western surveys and his work as director of the United States Geological Survey and as organizer of the Bureau of Ethnology were briefly described. At the close of the meeting the following vote was passed:—

The members of the Eighth International Geographic Congress who visited the Grand Canyon of the Colorado River on September 26 and 27, 1904, express the hope that a suitable monument may be erected on the edge of the plateau overlooking the Grand Canyon to commemorate the labors of John Wesley Powell as explorer, geologist and ethnologist; and they request that committee, consisting of Messrs. Davis (temporary chairman), Bryant, Day, Gilbert, Hill, Libbey, McGee, Salisbury and Walcott, with power to add to their number, take steps to carry the above suggestion into effect.

The record of the vote was signed by fifty-four members of the congress excursion. A meeting of the committee will be held in Philadelphia, on Friday, December 30, for the purpose of organizing and taking such action as may seem appropriate.

SCIENTIFIC NOTES AND NEWS.

Dr. R. S. Woodward, professor of mechanics and mathematical physics and dean of the faculty of pure science, Columbia University, was elected president of the Carnegie Institution at the meeting of the trustees held at Washington, on December 13.

At the meeting of the trustees of Princeton University on December 8 the resignation of Professor Charles A. Young from the chair of astronomy was accepted to take effect at the close of the present academic year, when he will become professor emeritus. Professor Young has held the chair of astronomy at Princeton since 1877. He celebrated his seventieth birthday on December 15.

The council of the Edinburgh Royal Society at its recent meeting, decided to award to Professor Sir J. Dewar, F.R.S., the Gunning Victoria jubilee prize for 1900–1904, for his researches on the liquefaction of gases extending over the last quarter of a century, and on the chemical and physical properties of substances at low temperatures.

M. Dastre, professor of medicine at Paris, has been elected a member of the Paris Academy of Sciences.

Dr. J. Mackintosh Bell, instructor in geology at Harvard University, a nephew of Dr. Robert Bell, F.R.S., acting director of the Geological Survey of Canada, has been appointed government geologist of New Zealand to succeed Sir James Hector.

The next autumn meeting of the Iron and Steel Institute of Great Britain will be held at Sheffield under the presidency of Mr. R. A. Hadfield.

Dr. Victor C. Vaughan, of the University of Michigan, addressed the Philadelphia Pathological Society on December 8, at the College of Physicians, on the 'Relation of Food Adulteration to the Public Health.' A reception was tendered to Dr. Vaughan after the meeting.

Dr. E. O. Hovey, of the American Museum of Natural History, lectured at the Massachusetts Institute of Technology on December 8, under the auspices of the Society of Arts, on 'Mont Pelé: the Eruptions of 1902 and the Growth and Destruction of the Great Spine, or Obelisk.'

DR. SVEN HEDIN, the Swedish explorer, is now in St. Petersburg, and will shortly deliver a lecture on the scientific results of his travels in Central Asia.

MR. ALEXANDER SIEMENS gave his inaugural address before the British Institution of Electrical Engineers on December 10.

THE Massachusetts Institute of Technology celebrated on December 7 the one hundredth anniversary of the birth of its founder and first president, William Barton Rogers.

A SUBSCRIPTION, headed by M. Albert Gaudry, has been started for a monument to M. Bernard Renault, well known for his valuable researches into the microorganisms of the coal measures, and especially of the bogheads and of the cannel coal of the United States, who died from overwork and in limited circumstances on October 14 last. The monument will be erected at his native place, Autun, where he is buried, and subscriptions should be sent to M. Berthier, secretaire de la Société d'histoire naturelle, 2, rue de l'Arbalète, Autun, France.

DR. GEORGE VIVIAN POORE, formerly professor of medicine in University College, London, died on November 23.

A DONOR, who desires to remain anonymous, has placed £1,000 in the hands of the treasurer of the Royal Society, £500 to be placed to the credit of the 'Catalogue of Scientific Papers Account' of the Royal Society, and the remainder to the credit of the 'National Physical Laboratory Account.'

THE Physico-Chemical Club of Boston and Cambridge has begun the third year of its membership with over sixty members. At the first meeting of the year, in November, Professor T. W. Richards, of Harvard, was elected president, in place of Professor A. A. Noyes, of the Massachusetts Institute of Technology, who has held the post for two years. Professor Noyes was made vice-president, and his colleague, Professor W. D. Coolidge, was made secretary and treasurer. Dr. G. P. Baxter described his important work on the atomic weights of iodine, and Mr. E. C. Kraus spoke of his interesting work on the remarkable properties of solutions in liquid ammonia.

THE trustees of the Wagner Free Institute of Science of Philadelphia announce that in view of the largely increased facilities provided within the past few years by the publication departments of the various institutions of learning, and more especially by the Carnegie Institution, for the promotion of original research with its incident publications, the Wagner Free Institute of Science has thought it wise to discontinue for the present its work in this department and to devote its energies more exclusively to other purposes indicated by its founder.

THE twenty-fifth annual report of the director of the United States Geological Survey is now ready. It contains 388 pages and is illustrated with 25 maps, which show the progress of topographic or geologic surveys made in different parts of the United States and Alaska. The general results achieved by the survey in the twenty-five years of its existence are summarized, and a *résumé* is given of the work accomplished by each of the survey's great divisions during the fiscal year from July, 1903, to July, 1904.

A PUBLIC museum has been incorporated at St. Louis. It is proposed to secure some of the objects exhibited at the St. Louis Exposition.

LADY FLOWER writes to the London *Times* that Mr. G. W. Duff-Assheton-Smith, whose death was recently recorded in *The Times,* stocked his park at Vaynol with many rare and interesting animals, including the large moose and small Japanese deer, old English white cattle, American bison, zebras, kangaroos and emus. One of the otters was so tame that when swimming in the lake it would come back when whistled to. Mr. Assheton-Smith was a courteous and kindly country gentleman, and his knowledge of animals and bird life was remarkable.

THE Bureau of Forestry has recently signed an agreement to make extensive timber seasoning tests in two western states, in cooperation with two telegraph and telephone companies. Experimental stations will be located

at Martinette, Wis., and Escanaba, Mich.; and probably a third station will be established at Ashland, Wis. The expense of the experiments will be borne jointly by the bureau and the companies. Cedar and tamarack telephone and telegraph poles will be furnished by the state of Wisconsin free of cost, and two railroad companies have agreed to haul them to the experiment stations without charge for freight.

UNIVERSITY AND EDUCATIONAL NEWS.

A gift of $50,000 by Mr. Edward D. Adams, in memory of his son, Ernest K. Adams, has been made to Columbia University for the foundation of a research fellowship in physical science. The gift is accompanied by a valuable collection of scientific apparatus to be allotted to the electrical, physical and psychological laboratories of the university.

It was made a condition of granting a charter to the University of Leeds that an endowment fund of at least £100,000 should be collected. Of this sum over $60,000 has been subscribed.

The Liverpool city council has voted the sum of £10,000 towards the support of the University of Liverpool.

By the will of Mr. G. T. B. Wigan, M.A., of Trinity College, Cambridge, a sum of above £9,000 was bequeathed to the university, the annual income to be applied for the purpose of promoting and encouraging scientific education or research in the university.

The University of Aberdeen will celebrate the four hundredth anniversary of its foundation in the summer of 1906.

The commissioner of agriculture for the Dominion of Canada, Mr. Robertson, will resign on January 1, to take charge of the new agricultural college established by Sir William MacDonald. According to the London *Times*, this will probably be the most complete agricultural college in the world. It is to be known as the Macdonald Foundation for Rural Education, the organization of which will be in the hands solely of Sir W. Macdonald and Mr. Robertson. It is to consist of three departments. The first will be for original research in bacteriology as applied to soils and products, in the biology of animals and plants, and in agricultural chemistry. The second will be a department of farms for the practical illustration of the discoveries in the research department, and the third will be a department of instruction combining a farm school and a college of agriculture. There will be residences for men and women students. The laboratories will be equipped in the most complete manner, and the best features of the principal agricultural colleges in Europe and America will be adopted. The amount of Sir W. Macdonald's benefaction is said on good authority to be not less than £1,000,000.

On the occasion of the installation of Lord Kelvin as chancellor of the University of Glasgow, on November 29, the degree of LL.D. was conferred on Dr. James Thomson Bottomley, F.R.S., Arnott and Thomson demonstrator in experimental physics in the University for 24 years from 1875; Admiral Sir John Charles Dalrymple-Hay, F.R.S.; Mr. Guglielmo Marconi; and the Hon. Charles A. Parsons, F.R.S.

Professor William G. Raymond, of the Rensselaer Polytechnic Institute, has been appointed professor of civil engineering and head of the departments of engineering in the Iowa State University at Iowa City. Professor Sherman M. Woodward, of the University of Arizona, has been called to the chair of steam engineering.

Professor Harris J. Ryan, of Cornell University, has been appointed professor of electrical engineering at Stanford University.

Mr. Andrew Carnegie has been reelected Lord Rector of the University of St. Andrews without opposition, Mr. Andrew Lang and Sir Henry Craik having declined to stand.

Augustus Edward Hough Love, M.A., D.C.L., F.R.S., Sedleian professor of natural philosophy at Oxford, formerly fellow of St. John's College, Cambridge, has been elected to an honorary fellowship at Queen's College.

Mr. E. Verner has been elected to a newly established chair of applied chemistry at the University of Dublin.